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ALEXANDER AGASSIZ, 1835–1910

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ALEXANDER EMMANUEL RODOLPHE AGASSIZ, only son of Louis Agassiz, was born at Neuchâtel, Switzerland, on December 17, 1835.

The great English statistician Galton found that men who attain eminence in science are nearly always sons of remarkable women, and Alexander Agassiz was no exception to this rule. His mother was Cecile Braun, the daughter of the postmaster general of the Grand Duchy of Baden, who was a geologist of note and the possessor of the largest collection of minerals in Germany. Cecile Braun was a woman of culture and an artist of exceptional ability, and she was the first who labored to illustrate the early works of Louis Agassiz, some of the best plates in the "Poissons fossiles" being by her hand. Her brother, Alexander Braun, after whom her son was named, was a distinguished botanist and philosopher, and another brother, Max Braun, was an eminent mining engineer and geologist, and the director of the largest zinc mine in Europe. Thus we find that intellectual superiority was characteristic of both the paternal and maternal ancestors of Alexander Agassiz.

After the birth of her son, sorrow came upon the family, for the heavy expenses demanded by the publication of Louis Agassiz's numerous elaborate monographs with their hundreds of illustrations had exhausted not only their author's means, but had drained the resources of the entire community of Neuchâtel in so far as they could be enlisted for the cause of science. Thus in March, 1846, Louis Agassiz was forced to leave Neuchâtel, and to begin the long journey toward America, where he found a wider field for his great endeavors. Before his wife or children could follow him to his new home, she died in 1848 after a lingering illness.

I cite these events because they show that the early youth of Alexander Agassiz was passed in a period of domestic confusion and sorrow which may have left its mark upon him throughout life, for his great self-reliance was a characteristic rarely developed in those whose early years have been free from care. Life was a severe struggle for him, and though his victories were great they were won after hard-fought battles.

After the departure of his father from Neuchâtel Alexander remained with his mother throughout the period of her failing health, and after her death his father's cousin, Dr. Mayor, and the Reverend Marc Fivaz brought him to America, where he rejoined his father in America in June, 1849, and entered the Cambridge High School in the autumn of the same year.

The earliest published picture of Alexander Agassiz is by his father's artist Dinkel and appears upon the cover of the first livraison of the "Histoire naturelles des Poissons d Eau douce de l'Europe Centrale" published in 1839. It shows him as a little boy of four years fishing upon the shore of the Lake of Neuchâtel.

In early life Alexander exhibited his independence of character and incurred the Prussian governor's displeasure and his father's reproof through his willful neglect to salute this official when he passed upon the opposite side of the street. He must also have shown his characteristic pertinacity, for before he came to America he could play well upon the violin, an accomplishment which he allowed to fall into abeyance in later years.

In the spring of 1850, soon after the arrival of Alexander in America, his father took for his second wife Miss Elizabeth C. Cary, of Boston, in whom he found a new mother throughout life, and he took the most tender care of her until her death long years afterwards, when he himself was an old man. Doubtless many of the finer traits of his rugged character were developed through the refining influence due to the care and teaching he received from this superior woman.

Nature and his father made him a naturalist, and his reverence for his father was almost a religion with him. He became the first student his father taught in America.

He entered Harvard College and graduated in 1855 with the degree of A.B., and then studied engineering, geology and chemistry in the Lawrence Scientific School, obtaining one B.S. in 1857, and another in natural history in 1862. During his college days he was much interested in rowing and was bow oar of the four-oared crew which won the race against Yale on the Connecticut River at Springfield on July 22, 1855, at which time he weighed only 145 pounds. He continued to row on the university crew until 1858, when the future President Eliot was one of his comrades in the boat.

After graduating from the Lawrence Scientific School he studied chemistry for a few months at Harvard, and then taught in his father's school for young ladies until 1859, when he was appointed an assistant on the U. S. Survey, and departed to take part in the task of charting the region of the mouth of the Columbia River, Oregon, and in establishing the northwest boundary. During this visit to the Pacific coast he found time in intervals of travel between official duties to study the fishes and medusæ of San Francisco harbor and Puget Sound, and to collect specimens at Acapulco and Panama for his father's museum; but after a year's absence he acceded to his father's earnest request and came home to Cambridge to continue his zoological studies and to assist in the upbuilding of the great museum which was the dream of his father's life.

We now come to the period of the beginning of his scientific productivity, for in 1859 he published his first paper—a brief address before the Boston Society of Natural History upon the mechanism of the flight of Lepidoptera. It seems strange that this first paper of one who was destined to devote his life to the study of marine animals and to the sea should have been upon butterflies and moths. Moreover, it is his only paper save one upon a mechanical principle underlying animal activity, his later work in zoology being of a systematic, descriptive or embryological character.

These years when he worked by his father's side and assisted him from the time the museum was formally opened in 1860 until 1866 when he went to Michigan to develop the Calumet and Hecla copper mine were probably the happiest of his life. At first he had charge of the alcoholic specimens, of the exchanges and the business management of the museum—sufficient to swamp an ordinary man; but he was a hercules of energy and executive power, and his remarkable ability as an organizer probably saved the museum from many an embarrassment which his father's buoyant enthusiasm and simple faith in destiny might have brought upon it. He had much of that ardent love of the study of nature which was his father's own, but it was tempered and controlled by a more conservative judgment and a keener insight into the motives of men, so that the two working in sympathy together made an ideal team for drawing the museum upward from obscurity to prominence; for these early days were critical ones in its history. In 1866, when his father was absent in Brazil, Alexander Agassiz had entire charge of the museum.

On November 15, 1860, he married Miss Anna Russell, daughter of George R. Russell, a leading merchant of Boston. The wedding took place at the home of the bride's brother-in-law, Dr. Theodore Lyman.

Arduous as his official duties were from 1859 to 1866, when he studied in the museum at Cambridge, they did not prevent his accom-

plishing a remarkable amount of work in science, for he devoted his summers to study upon the seashore at a time when the waters of many a now polluted harbor were pure, so that he discovered many new and remarkable marine animals in the neighborhood of Boston, where now nearly all aquatic life has disappeared. He produced eighteen publications during this period, the most notable being his illustrated catalogue of the "North American Acalephæ," containing descriptions of many new and interesting forms of medusæ from the Pacific and Atlantic coasts, and illustrated by 360 figures drawn from life by his own hand. It is but a just tribute to his thoroughness as a collector and observer to say that some of these medusæ have never again been seen since he discovered them off the New England coast fifty years ago.

Another interesting paper of this period is his "Embryology of the Starfish" of 66 pages illustrated by 8 plates containing 113 figures beautifully drawn from life by the author; and yet another paper is upon the young stages of annelid worms in which he shows that in past ages adult worms were often provided with very large bristles, and that the young of existing marine worms still have such structures.

At this time also he wrote much upon echinoderms, and made substantial progress upon that great work of his early manhood, the "Revision of the Echini," which finally appeared in four parts between 1872–74 and consists of 762 quarto pages of text and 94 plates; composed of drawings and photographs made by the author. This work caused his father keen delight, for he foresaw that it portended a distinguished career in science to his gifted son. It won the Walker prize of $1,000 from the Boston Society of Natural History, and brought to its young author an international reputation.

In 1866 he was elected to membership in the National Academy of Sciences, which at that time recruited itself from the active young workers of the country. He was president of the academy from 1901 to 1907, and its foreign secretary from 1891 to 1901 and from 1908 until his death in 1910. He bequeathed $50,000 to the academy. He was also deeply interested in the American Academy of Arts and Sciences and served as its president, gave large sums to it and left it $50,000 after his death. These two academies were the only scientific associations of America in which he took any active interest.

Between 1860 and 1866 he laid the foundation for all that he was to achieve in science, with the exception of his elaborate explorations of coral reefs, and, with this exception, all of the subjects which were to engross his attention in future years were then engaging his active interest. He never departed from the thought and method of these early days, and he always spoke of them with loving remembrance as "the good old days"—their influence upon his scientific career was paramount. For example, he never adopted the methods of the his-

tologist, which were not used by his father, and he confined himself to the study of living animals whenever this was possible. Thus it is that he ranks among the foremost of those systematists and embryologists who have devoted themselves to the observation of marine animals, but histology was wholly neglected by him. Nor did he ever take part in that stirring discussion of Darwinism which engrossed the attention of all of his contemporaries. It would be unfair to say that he did not believe in evolution, but the truth is that he was but little interested in the speculative side of science, excepting in so far as its deductions could be based upon observations of facts. In later life he came to regard the labors of the physiologist and of the laboratory experimenters upon the reactions of animals as beyond the scope of zoology.

But the walls of the museum and problems of zoology were too narrow a bound for such a genius of activity as Alexander Agassiz; moreover, he was poor and he required funds for the prosecution and publication of his work in science and thus in 1865 he engaged in coal mining in Pennsylvania, and in the following year he temporarily left the museum and became superintendent of the then unprofitable Calumet copper mine on the southern shore of Lake Superior, and in 1867 he united the Calumet with the adjacent Hecla mine, calling the combined property the Calumet and Hecla. It is due more to him than to any other man that this mine has produced the largest profits ever divided by any incorporated mining company, for the dividends up to December 31, 1907, amounted to $105,850,000. From the first days of his leadership in its affairs the company excelled all other mines in the introduction of heavy machinery and modern methods. Indeed its life depended upon the development of methods of mining upon a large scale, and so vastly has it grown that 83,863,116 pounds of fine copper were produced in 1907. As superintendent and director and afterwards as president of the company Alexander Agassiz steadily pursued the policy which led to this extraordinary industrial success, and out of the wealth it brought him he devoted upward of $1,000,000 to forwarding the aims of the museum which his father had founded, until he made it famous throughout the world for its excellent publications in science. He also expended large sums upon numerous scientific expeditions, the results of which he published in a manner that has never been excelled.

To have developed the greatest copper mine in the world would have taxed the entire energy of many an able man, but so extraordinary was Alexander Agassiz's capacity for productive labor that he became the sole author of 127 notable scientific works, many of them large books with numerous plates and illustrations drawn by himself, and he published many other minor papers. He was also the joint author of 18 and the patron or inspirer of more than 100 more which were written by specialists in America, Europe and Japan to whom he sent the collections he had gathered.

In his treatment of assistants and collaborators he displayed a most commendably unselfish spirit, and indeed the only differences I experienced during eight years in which I served as his assistant were occasioned in persuading him to permit his name to appear as the senior author of publications which were actually the result of our joint efforts.

Labor at the copper mines made enormous drains upon his seemingly inexhaustible energy, for during the early years of his connection with the company he worked upon an average of fourteen and a half hours each day. Yet arduous as these duties were between 1867 and 1874 they made but little difference in the output of his scientific work, for in this period he produced 19 papers, one of them being his famous "Revision of the Echini." Another announces the discovery that *Tornaria* is undoubtedly the larva of *Balanoglossus*, and in another he proves that the peculiar pincer-like organs found upon the echini are in reality only highly modified spines, and they serve to keep the animal clean by actually grasping and removing detritus from the surface of the creature. In another work of this period he presents a paper illustrated by 202 excellent figures and giving a complete account of the embryology of those most diaphanous of marine animals, the Ctenophoræ.

Indeed it may be said that while his later work was far more elaborate and widely known, it was not more brilliant than that of this period which closed with his fortieth year, and these older papers are of such fundamental importance that they are quoted in all general text-books of zoology. We see then that these days of his early manhood between 1861 and 1873 were rich in achievement in science, and remarkable in other respects, for it was during this period that he raised himself from poverty to wealth more than sufficient to meet the demands of his expensive researches in zoology.

But the "happy old days" were soon to pass away forever from the life of Alexander Agassiz, for on December 14, 1873, his great father died, and to deepen his misery his wife to whom he was devotedly attached passed away only eight days after his father's death, and his own health, undermined by too strenuous labor, failed so seriously that throughout the remainder of his life he suffered from an impairment of the circulation which obliged him to seek a warm climate every winter.

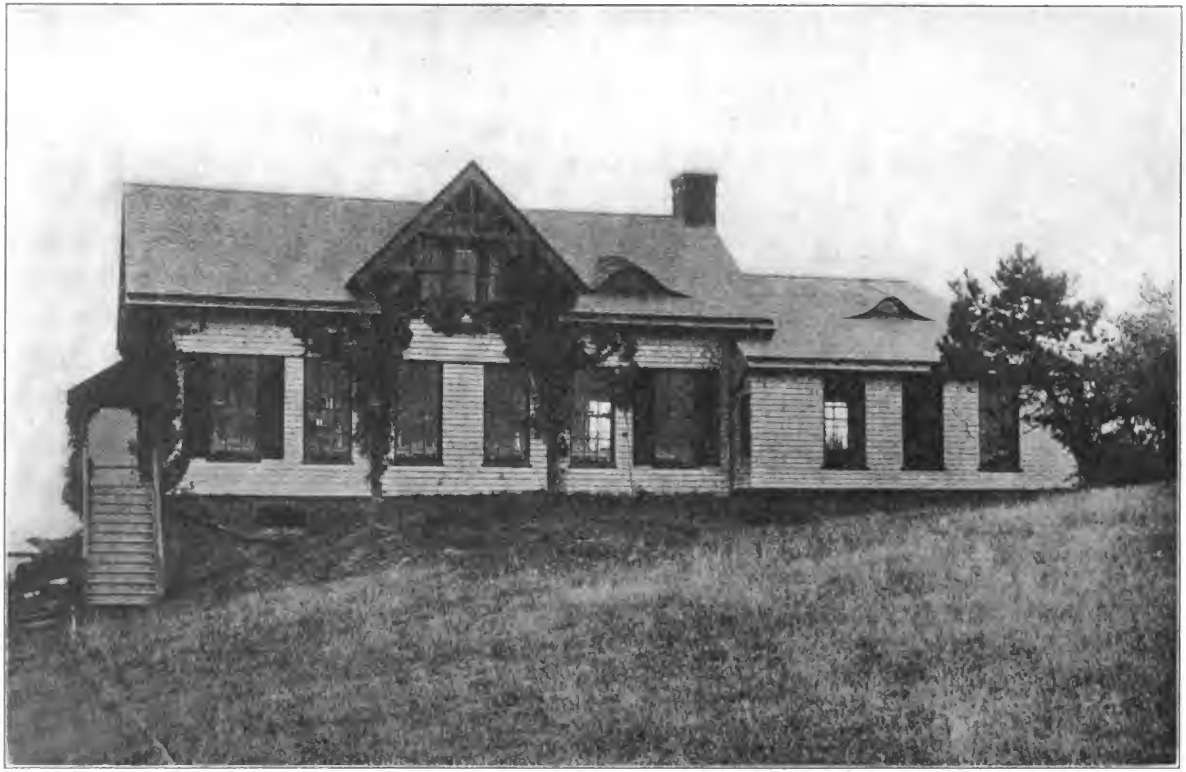
Those who knew him in his happier years say that from this time onward a great change was observed in him. These irreparable losses came upon him at a time when youth was gone, but middle age had hardly come upon him and most things of life were yet in store for him. Henceforth he was to live alone with his sorrow, master always of himself, simple almost to austerity in his tastes, but deprived of that sympathy which only a wife could give, it is but little to be wondered at

that he raised a wall between himself and the great unsympathetic world, which only those nearest to him and a few most intimate scientific associates could penetrate. In early life he had been buoyant in spirit, popular and beloved by all who knew him, but after the sorrows of 1873 he withdrew from broader contact with the world, and while he still remained cordially intimate with a few of the greatest leaders, from the rank and file of scientific men he held himself far and aloof. One must always bear the fact in mind that during the last thirty-seven years of his life he was a saddened and an ill man—one whose deepest love was buried and whose fondest hopes had been wrecked. We must also consider that a tendency toward this reserve probably came to him through inheritance from the German blood of his mother's side of the house, and it may in some measure be accounted for by the fact that English always remained a foreign tongue to him, for he thought in French, and in temperament he remained European rather than American.

Yet among scientific men he became the greatest patron of zoology our country has known. In 1910, at the time of his death, the fifty-fourth volume of the "Bulletins" and the fortieth volume of the "Memoirs" of the Museum of Comparative Zoology were appearing. These publications had been started in 1863 and 1864, and in the number of important and beautifully illustrated papers they contain they have been excelled by only a few of the most active scientific societies of the world; yet the expense of producing them has largely been borne by one man—Alexander Agassiz.

In 1870–71 he visited many European museums to study specimens of echini for his great work upon this group and he was also especially interested in the results of the English deep-sea dredging expeditions in the *Porcupine,* little dreaming that he was himself to become a great leader in such work.

In 1873 when Mr. John Anderson, of New York, offered his father the Island of Penikese as the site for a marine biological laboratory, Alexander Agassiz used all his efforts to dissuade him from its acceptance, but failing in this he served for the first year as an instructor and the second as superintendent of the school. He gives a history of this experience in an article in 1892 in THE POPULAR SCIENCE MONTHLY, volume 42, p. 123. Mr. Anderson's final loss of interest in the laboratory and his refusal to consent to its removal to Woods Hole led to its abandonment. Although Alexander Agassiz, prompted by his deep interest in marine zoology, did not give up the attempt to maintain the school until after an appeal for aid addressed to the superintendents of public institutions and presidents of state boards of education throughout the United States had met with inadequate response. Then he himself paid the expenses and the Penikese School passed out of existence.

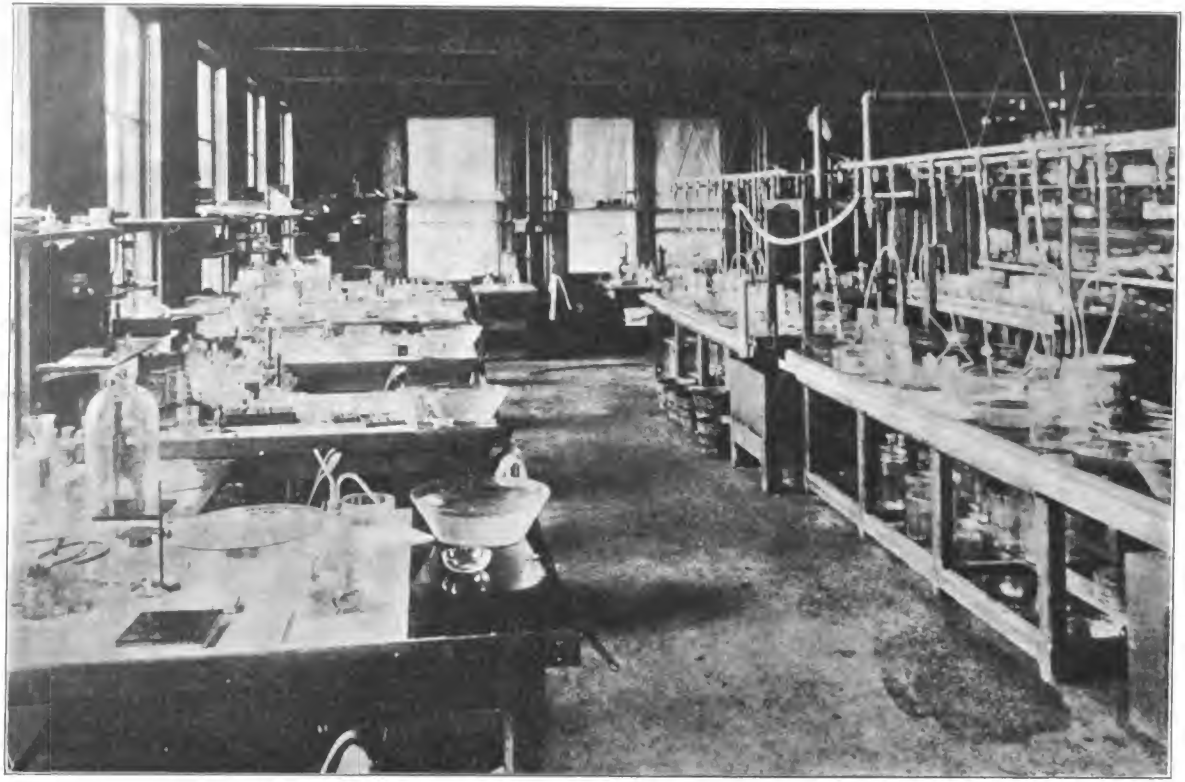


THE AGASSIZ LABORATORY AT NEWPORT, RHODE ISLAND.
From the Annual Report of the Curator of the Museum of Comparative Zoology at Harvard for 1891-92.
Kindness of Dr. Samuel Henshaw.

His experience at Penikese was, however, by no means in vain, for it deeply impressed him with the advisability of establishing a summer school for research in marine zoology, so that in 1877 he built upon his place at Castle Hill at the mouth of Newport Harbor an ideal little research laboratory which afforded excellent accommodations for half a dozen students at a time. For eighteen years students and instructors from Harvard College visited this charming spot, and many are the papers which resulted from their labors there. Count Pourtalès, W. K. Brooks, Fewkes and Whitman were the first workers in the station, and each year about ten of the most promising of the research students in zoology at Harvard were privileged to study at the Newport Laboratory. Every day a stage bore them from the town, four miles away, to the laboratory and back again at five o'clock in the afternoon, after the daily swim in the ocean. The laboratory was excellently equipped with reagents, glassware and large tanks provided with running salt or fresh water. The microscope tables were set upon stone foundations to avoid vibration, and a good little steam launch lay at her moorings in a near cove ready to dredge in the service of science. I treasure the memory of those youthful days at Newport when the enthusiastic spirit of our great leader was an inspiration to each and every one of us, and I recall his delight over the rare "finds" we occasionally discovered in the surface tow which was made every night and lay awaiting our study in the morning. Gradually, however, a change came over the Newport Laboratory, the once pure water of the harbor became more and more polluted as population and shipping increased until finally in 1897 students were no longer invited to come to Newport, and the scientific existence of the laboratory ceased. An account of the laboratory together with a plan of the building will be found in *Nature*, volume 19, pp. 317–319, 1879, and in the *Century Magazine* for September, 1883, but these fail to give an idea of the attractive little vine-clad building nestled down on the slope of the shore overlooking its little cove with the beautiful bay to the northward and the ocean on the south.

Alexander Agassiz was the first to see that the southern shore of New England was most favorably placed for the site of such a station, for he discovered that here arctic forms are carried down during the winter and early spring, whereas late in summer the southerly winds bring drifting upward from the Gulf Stream animals whose true homes are in the warm waters of the tropical Atlantic, and thus one meets with an extraordinary seasonal variation of marine life on the southern coast of New England.

In 1874 Alexander Agassiz was elected curator of the museum to succeed his father in this responsible position, and indeed the prospects of the museum were at that time such as to inspire grave apprehension,



INTERIOR OF THE AGASSIZ LABORATORY AT NEWPORT.
From the Annual Report of the Curator of the Museum of Comparative Zoology at Harvard for 1891-92.
Kindness of Dr. Samuel Henshaw.

for its annual income was but $10,000 while it had a debt of $40,000, and only four fifths of the north wing was completed. Fortunately, however, the devotion of the country to the memory of the great Louis Agassiz was such that the museum was not allowed to fail as had the school at Penikese. Over $310,000 were raised by popular subscription and through state grants for the support of the museum, and as a memorial to Louis Agassiz, $25,000 being contributed by Alexander Agassiz himself. It is interesting to see that $1,215 of the amount was subscribed by 1,233 workmen of the Calumet and Hecla, although there were at that time not more than 1,400 men at the mine.

From 1874 Alexander Agassiz remained the actual, although not constantly the nominal, head of the Museum of Comparative Zoology, and from 1902 until his death in 1910 he bore the title of director of the Harvard University Museum.

The growth of the museum building was slow but constant. Alexander Agassiz himself completed the construction of the zoological section in 1882 and other public-spirited men and women, including his two sisters, contributed to build other parts of the edifice, until at present only one hundred feet of the southern wing of the building planned so long ago by Louis Agassiz remains to be completed. The total cost of the building has been more than $1,200,000, and its invested capital amounts to somewhat more than $900,000. Thus while it is much hampered for funds it still remains the greatest *university* museum in the United States. The zoological section has been greatly enriched by collections gathered by Alexander and Louis Agassiz, and their gifts to the library have placed it in a position in which it is unsurpassed in America, more than 6,000 bound volumes having been presented by Alexander Agassiz himself.

In the classification of its zoological exhibits the museum is one of the clearest existing models of the system of Cuvier, for it must be remembered that intellectually Louis Agassiz was Cuvier's son, and Alexander Agassiz steadfastly pursued his father's plan in so far as the museum's exhibits were concerned.

No family has striven more effectually for the intellectual uplifting of Harvard than that of the Agassiz, and it is to be regretted that the great museum which they founded and fostered does not officially bear their name, but instead is described by an almost meaningless phrase, "The Museum of Comparative Zoology."

Alexander Agassiz was a loyal son of his *alma mater* and he served as an overseer of Harvard from 1873 to 1878 and again in 1885; and he was a fellow from 1878 to 1884 and from 1886 to 1890. In 1885 the university conferred upon him the honorary degree of LL.D.

The year 1875 marks the beginning of Alexander Agassiz's career as a leader of expeditions, for with Dr. Samuel Garman as his assistant

he explored Lake Titicaca and the coast region of Peru and Chile. From this time onward until the close of his life exploration was to engross more and more of his attention, to the final exclusion of the embryological studies that had given color to his earlier years. The last publication in which he records the results of the rearing of animals is his joint paper of 1889 with Professor Charles O. Whitman and is upon the development of fishes. After 1889 he gave up the raising of larvæ in his aquaria at Newport, and became an explorer, geologist and systematic zoologist; although it should be said that the last paper published during his lifetime is a short one upon the temporary existence of a lantern and of teeth in the young *Echinonëus*. It is, however, based upon the study of museum material, and records an observation made by A. M. Westergren.

His remarkable energy and executive ability fitted him in an eminent degree to be the leader of scientific expeditions. Each exploring trip was planned to a day even to its minute details, every course charted, distances measured and every station decided upon, before he left his desk in the Harvard Museum, so that all of its achievements were actually prearranged. At times it was of vital import to his expeditions to have supplies of coal brought to some distant island in the tropics, but invariably when he arrived his colliers would have preceded him, and all went forward with clock-work regularity. In fact, before starting he read all that was to be found upon the regions he designed to visit so that he was enabled to begin the writings of his results the moment the voyage was over. It is due chiefly to his forethought that in more than 100,000 miles of wandering over tropical seas he never met with a serious accident; and this is the more remarkable when one considers that in order to land upon the coral reefs he was forced to cruise in the hottest season when the brooding calms were liable at any moment to break into a hurricane. Day after day I saw him remain upon the bridge of the steamer sketching salient features of many a lonely coast that he of all naturalists was the first to see. The rolling of the vessel caused him acute distress, yet though sea-sick he worked on undaunted for the keynote of his character was pertinacity.

As we have said his first expedition was to South America to explore Lake Titicaca and to visit the copper mines of Peru and Chile. He published a hydrographic chart of the lake, sounded its depths, determined its temperature, collected its animals and plants, and relics of the ancient Peruvians who once lived upon its islands. Among other results he found at Tilibiche, Peru, a reef of fossil corals elevated 2,900–3,000 feet above the sea and 20 miles inland from the ocean, thus showing that the recent elevation of some parts of the western coast of South America has been even greater than had been observed by Darwin.

Upon returning from South America, his embryological studies were resumed at Newport, and the development of flounders and other young fishes interested him especially. It was well known that in the young flounder the eyes are on both sides of the head and that after the fish falls over on one side, the eye of the lower side travels around and comes to lie beside its fellow on the upper side of the fish, but Alexander Agassiz discovered that in the transparent young of flounders allied to the Plagusiæ the lower eye actually penetrates through the tissues of the head and reappears on the surface of the upper side of the fish.

In the young of other bony fishes he discovered a caudal lobe showing that in an early stage the tails of the bony fishes resemble the adult tails of the more ancient ganoids.

He also found that under the skin of flounders there are yellow, red and black pigment cells and that changes of color are due to the independent expansion or contraction of these several cells; and in 1892 he made the interesting discovery that if young flounders be placed for six weeks in aquaria with white surroundings they lose nearly all color and do not regain their normal color, even if at the end of this time they be surrounded by black.

These studies of fishes, begun in 1875, were continued for many years in the intervals between expeditions, the last of the series being published in 1892. One of the most important papers of this series appeared in 1878 and is upon the development of that archæic fish the gar pike, *Lepidosteus*.

But of all animals the echinoderms interested him most deeply. Indeed of the 145 most important scientific papers of which he was sole or joint author 45 treat of echinoderms. Accordingly in 1874–77 we find him actively engaged in their study. In 1874 he announces the discovery that hybrid larvæ may be produced by artificial means between the two common species of starfish of the New England coast. In 1876 he studied the structure of some viviparous echini from the Kerguelen Islands, and found that they habitually carried their young about with them until the young had acquired most of the characters of the adult. In 1877 his beautifully illustrated work upon North American starfishes was published.

In 1876 he was keenly interested when he visited Sir Wyville Thomson in Scotland and inspected the vast collections of deep-sea forms brought home from the three-years cruise of the *Challenger;* and it was a happy moment for him when in 1877 an arrangement was perfected with the United States government by virtue of the terms of which he was given the scientific direction of the U. S. Coast Survey steamer *Blake* during the entire time of her purposed explorations of the West Indian and Gulf Stream region. He joined the *Blake* at

Havana, Cuba, in December, 1877, and remained on board until April, 1878, exploring the Gulf of Mexico, and adjacent regions. Admiral, then Lieutenant Commander C. D. Sigsbee, U.S.N., was in command, and his ingenious inventions of sounding apparatus, trawls, etc., enabled the expedition to accomplish unprecedented results.

The second cruise of the *Blake* started from Washington on November 27, 1878, with Captain J. R. Bartlett, U.S.N., in command, and throughout the winter of 1878–79 they cruised among the Windward Isles of the West Indies and over the Caribbean Sea, visiting Havana, Jamaica, Hayti, Porto Rico, St. Thomas, Santa Cruz, Montserrat, St. Kitts, Guadeloupe, Dominica, Martinique, St. Lucia, St. Vincent, Granadines, Grenada and Barbados, and gathering an immense collection of animals from the depths of the ocean.

The third and last cruise of the *Blake* was for the purpose of sounding the depths of the Gulf Stream. They started from Newport in June and cruised until August, 1880, running seven lines of soundings off the coast between Charleston and George's Bank, which led to the discovery that a plateau covered by water not more than 600 fathoms deep extends from the Bahamas northward to Cape Hatteras, forming a vast triangular area of shallow water, the outer edge of which is from 300 to 350 miles out in the ocean from the coast of the Southern Atlantic States. The Gulf Stream flows across this area on its course between the Straits of Bemini to Cape Hatteras, and the outer edge of this shallow bank is where the North American continent rises abruptly from the depths of the flat floor of the ocean. The name "Blake Plateau" was most appropriately given by Alexander Agassiz to this extensive area of shallow water.

During her three cruises the *Blake* made 355 soundings, deep-sea temperature observations, and trawl hauls yielding a phenomenally rich harvest of new and interesting marine animals. Among other things, the second cruise led to the discovery of a vast submarine valley, the "Bartlett Deep," extending for nearly 700 miles along the southern coast of Cuba toward Honduras. Twenty miles south of Grand Cayman this great depression is 3,400 fathoms deep, so that the summits of the mountains of Cuba only 50 miles away are 28,000 feet above its somber trough.

This experience upon the *Blake* was the most momentous event in Alexander Agassiz's scientific life, for it gave him a taste for marine exploration which was to dominate his future career. Without this he might have continued to be an embryologist and systematic zoologist, but he was destined to more conspicuous achievements as an explorer.

Its effect upon the history of the Museum at Cambridge was also profound, for the output of museum publications had been so slow that at the end of 1877 only three volumes of the "Bulletins" and five vol-

umes of the "Memoirs" had been completed, and yet these publications had been appearing in parts for fourteen and thirteen years, respectively. The reports upon the great collections gathered by Alexander Agassiz's expeditions gave these museum publications an enormous impetus, so that at the time of his death in 1910 the fifty-fourth volume of the "Bulletin" and the fortieth of the "Memoirs" were appearing.

Alexander Agassiz realized that the government had always failed to provide adequately for the publication of the results of its many explorations, and thus he himself assumed the direction, and defrayed the entire expense, of all of the publications resulting from expeditions under government auspices of which he was the scientific director. No results of explorations have been more appropriately published or better illustrated than those under the auspices of Alexander Agassiz.

Alexander Agassiz did most wisely also in sending the various collections not only to specialists in America but to the leading students in Europe and Japan, thus securing the cooperation of those best competent to pronounce upon them.

During the first cruise of the *Blake* he discovered that the prevailing winds blowing over the Gulf Stream caused a marked concentration of floating life upon its western edge, and that this aggregation was nowhere richer than at the Tortugas, Florida. Accordingly under government auspices he visited the Tortugas in March and April, 1881, with Dr. J. W. Fewkes as his assistant. Although greatly hindered by stormy weather, he succeeded in securing a large collection of marine animals, notably the Porpitidæ and Velellidæ, an elaborate and fully illustrated account of which he published in 1883; and in the same year in the "Memoirs" of the American Academy he presented the results of his studies of the fine coral reefs of the Tortugas.

His "Blake" Echini appeared in 1883; and in 1888 came his last "Blake" publication, a general account of her three notable cruises. This crowning work comes nearer to being a popular book than anything he, as sole author, ever published. It is a general review of the results of the *Blake's* voyages between 1877 and 1880, and it appears in volumes 14 and 15 of the "Bulletins" of the Museum of Comparative Zoology, being illustrated by 545 maps and figures of the highest artistic and scientific merit.

It is rarely indeed that the results of exploration have been thus summarized in a single work, and none gives a clearer idea of the strange forms of the creatures that live upon the cold dark floor of the deep-sea than does this one.

The results may be significantly summarized by stating that we now know more of the topography and of the animals of the depths of the Gulf Stream and West Indian region than of any submarine area of equal extent in the world, and that this knowledge is due to the ex-

plorations of the *Blake* under Alexander Agassiz's scientific direction. It is but just to add that these notable achievements would have been impossible had it not been for the inventive genius and intelligent interest of Captain Sigsbee in devising sounding apparatus and trawls.

We now come to the closing period of Alexander Agassiz's scientific life—his long years of exploration of the coral reefs of the world, for during the winter of 1885 he visited the Hawaiian Islands, studying the reefs of Oahu, Maui and Hawaii.

For twenty-five years this study of the mode of formation of coral islands was to engage his rapt attention, and he was destined to wander farther and to see more coral reefs than has any man of science of the present or the past. His boyish joy upon the sight of some rare creature of the sea was something not altogether his own, for he inherited it from his father. The years of toil and care were all forgotten when he drifted in the mirrored waters above the reef and gazed downward into its world of subtle color where contrasts of olives, browns and greens were accentuated by a butterfly-like flash of brilliancy as some fish of the coral world glided outward from the depths of the shaded cavern.

He saw more coral reefs than has any living man and this very virtue of his exploration is its chief fault, for the study of coral reefs is a complex problem and it can not be solved by a superficial inspection such as he was forced to make. No one realized this more fully than he did himself, but he believed that the subject should be approached by a superficial survey of all of the reefs of the world, and thus he might hope to discover places where the problem might afterwards be studied with decisive results. He aimed to point out only the broad aspects of the problem, leaving the elucidation of details to those who might follow him.

I believe that science will come to see that he succeeded in showing that Darwin's simple explanation of the formation of atolls does not hold in any part of the world. Darwin, it will be remembered, assumed that wherever we find a volcanic mountain projecting above the sea in the tropical regions corals will grow upon its submerged slopes and form a ring around it. If then the mountain slowly sinks beneath the sea the corals will as constantly grow upward toward the surface, so that after the mountain has disappeared the atoll-ring of coral reefs will still remain.

Alexander Agassiz maintains, however, that atolls are formed in a variety of ways, and may develop where there has been neither marked elevation nor subsidence in modern times, as at the Great Barrier Reef of Australia, or under stationary conditions after a past period of elevation, as in the Fiji Islands, or by the dissolving away of the inner parts of an elevated limestone island as at Bermuda, or Fulangia in

Fiji, or we may have a submerged crater the volcanic rim of which may erode away to beneath sea-level, thus giving a foundation for a ring-shaped coral reef.

Unfortunately the very multitude of Alexander Agassiz's observations, and the somewhat confused style of his writing, renders him difficult to follow. Had he enjoyed greater experience as a lecturer he might have become a clearer writer, for he constantly assumed that his readers were as familiar with the subject as himself, and that a few words would make his meaning as clear to them as to him.

It is to be regretted that of the three great writers upon coral reefs Darwin saw only one atoll, Dana sailed past many but was permitted to land upon few, for the islands were then inhabited by dangerous cannibals, and Agassiz was compelled to cover such a vast field that certain of his conclusions, as he states himself, are still tentative; for the solution of some of the questions presented by these problems demands a more intensive and prolonged study than he was able to devote to them.

While in the Hawaiian Islands in 1885 he found that the coral reefs have repeatedly been buried under lava floes, and that the corals have again grown over the submerged lava. The reefs have nowhere been elevated more than 25 feet above sea-level, but the coral sands and shell fragments have been blown upward along the mountain slopes and have formed limestone dunes which the rains have cemented into solid rock. These wind-blown limestone ledges may be found 700 feet or more above the level of the sea.

In 1890 he published a paper showing that reef corals may become two and one half inches thick in less than seven years, his observations being based upon a study of corals that had grown upon the Havana-Key West cable.

In 1887 Alexander Agassiz was invited by the U. S. Fish Commission to assume the scientific direction of an expedition of the steamship *Albatross* between Panama and the Galapagos Islands, but he was unable to accept until 1891, when from February until May he cruised with the *Albatross* from Panama to Point Mola, thence to Cocos, Malpelo and Galapagos Islands, and from Acapulco to the Gulf of California, making 84 deep-sea trawl hauls, soundings and temperature observations, and in five more stations using the surface and sub-marine nets.

A significant feature of this expedition was due to the invention by Lieutenant Commander Z. L. Tanner, U.S.N., of a self-closing net which enabled one to obtain marine animals at any stratum of depth, and thus to determine the range in depth of marine creatures. The use of this excellent net led Alexander Agassiz to conclude that the floating life of the surface of the sea does not sink to a depth greater

than 200 fathoms, and that the bottom forms of the deep-sea do not rise more than 60 fathoms above the floor of the ocean, and that there is practically no life between 200 fathoms below the surface and 60 fathoms above the bottom. His later studies have, however, shown that these conclusions must be modified, for in the tropical Pacific surface forms are sometimes taken at a depth of about 300 fathoms beneath the surface, and although the surface animals do not commonly sink to depths greater than this, there is apparently a most interesting intermediate fauna of medusæ, etc., which are sometimes found at depths greater than 400 fathoms and which rarely or never rise to the surface. Agassiz clearly saw the complexities and difficulties of this problem, and realized that its solution can be reached only after many have labored upon it. Indeed, he himself was forced through lack of time to abandon its study to others.

A very rich collection of deep-sea forms then new to science was made by this expedition of the *Albatross* and have been described in numerous papers in the "Bulletins" and "Memoirs" of the museum at Harvard.

The most important general result was Alexander Agassiz's discovery that the deep-sea animals of the Gulf of Panama were more closely allied to those of the depths of the Caribbean Sea than the Caribbean forms were to those of the deep waters of the Atlantic. This leads him to conclude that the Gulf of Panama was once more intimately connected with the Caribbean than the latter is with the Atlantic, and thus the Caribbean Sea was at one time merely a bay of the Pacific, and has become shut off since Cretaceous times by the uplifting of the Isthmus of Panama.

In 1892 Alexander Agassiz published his general report upon this important exploration of the Panamic region, and he concludes that the Galapagos Islands have never been connected with the mainland of America, but that the ancestors of their peculiar animals and plants were drifted over the ocean by the prevailing winds and stranded upon the shores of these remote islands. He also observed that the animals of the deep sea are preponderatingly reddish or violet in color, and that blue-colored forms, such as are observed on the surface, are rare in the depths. This inclines him to suspect that the lingering remnant of sunlight which penetrates into the depths is red, but in view of the absence of observation he is cautious in advancing this suggestion.

Another paper of 1892 is his description of an interesting crinoid from the depths of the sea near the Galapagos Islands. This is a highly generalized form, and it is beautifully painted from life by Westergren, who accompanied him as artist upon the *Albatross*. In 1898 and 1904 he describes the deep-sea echini found off Panama, this

being his last paper upon the results of the explorations of 1891. The final report is beautifully illustrated with drawings made by A. M. Westergren.

In the autumn of 1892 his friend Mr. John M. Forbes offered to place at his disposal his steam yacht *Wild Duck,* a sea-worthy little vessel 127 feet long upon the water line; and from January until April, 1892, he cruised in this yacht, wandering for more than 4,500 miles among the Bahamas and off the Cuban coast, engaging in the study of the part which corals have played in the formation of these islands. On this and all subsequent expeditions he was accompanied by his son Maximilian, who was his father's constant companion and friend, and who served as his photographer. The results of this voyage were published in 1894 in the "Bulletin" of the Museum of Comparative Zoology.

He concludes that the Bahama Islands are composed of æolian rock, being formed of wind-blown fragments of shells and other limestone particles of animal origin which, after being blown upward above sea-level, have been agglutinated into rock by the agency of rain water. After being thus built up the islands subsided about 300 feet, and are now much smaller than they originally were, for the sea and atmospheric agencies have eroded them greatly. The present-day corals form a mere veneer over this submerged æolian rock and do not play a prominent part in forming the islands. The so-called "lagoons" of the Bahamas are merely parts of the interior of the islands which have been dissolved out under atmospheric agencies, rain, etc., and have been deepened by the action of the sea after the ocean water entered them. Hogsty Atoll he would regard as a plateau of submerged æolian rock surrounded by a rim which does not reach the surface and is protected from marine erosion by a coating of modern corals.

Five superimposed limestone terraces are seen at Cape Maysi and can be traced for a considerable distance along the Cuban coast. The lowermost of these terraces is raised only about twenty feet above sea level and is clearly an elevated coral reef, but the older and higher terraces he is inclined to regard as being of limestone covered only by a mere veneer of corals or containing only a few scattered coral heads and not true elevated coral reefs.

The peculiar flask-shaped harbors of Cuba with their narrow entrances and broad lagoons interested him greatly, and he decided that when the land was elevated these depressions had been leached out in the limestone by the action of streams in the drainage areas of the valleys, and when the land afterwards sank the broad valleys were submerged, with only a deep narrow entrance connecting them with the sea. Yumuri Valley would constitute just such a harbor were it submerged beneath the sea.

This study of the reefs of Cuba and the Bahamas naturally led him to renew his observations in Florida and to visit the Bermudas. He saw the Bermuda Islands in March, 1894, and in December of the same year he chartered a tug and steamed along the Barrier Reef of Florida.

He found that in common with the Bahamas the Bermudas consist of æolian limestone. In places the interior of these islands were dissolved away by the action of rain-water rendered acid by decomposing vegetable matter, and thus depressions were formed in the central parts of the islands. Then when the islands sank the sea broke through the rims and filled the lagoons, afterwards deepening them by its scouring action.

Thus the Bermudas have assumed an atoll-like shape, but their contour is not due to corals. Indeed, there are but few corals at Bermuda, and these form a mere veneer over the sunken æolian ledges. The so-called miniature atolls are mere pot-hole basins which have been scooped out by wave action in the æolian rock, and their rims are never more than eighteen inches high, and consist of a wall of æolian rock covered by a coating of serpulæ, algæ and corallines which enable them to withstand the wearing action of the sea. Thus Darwin's theory of coral reefs can not explain the conditions seen in the Bahamas and Bermudas.

The results of his study of the Florida Reef were finally published in 1896 in cooperation with Dr. Leon S. Griswold. Agassiz concludes that the Marquesas, of Florida, are not an atoll, but enclose a sound that has not been formed by subsidence, but by the solvent and mechanical action of the sea. Thus the Marquesas are similar in their geological history to other sounds back of the line of the Florida Keys.

He found an elevated reef extending along the seaward face of the Florida Keys from Lower Matacumbe to Soldier's Key. We now know, however, that the elevated reef actually extends from the southern end of Big Pine Key to Soldier's Key. Agassiz believed that the oolite limestone back of the elevated reef and along the mainland shore of Key Biscayne Bay was æolian rock; but Griswold decided that it was only a mud-flat which had been formed beneath the water, and afterwards elevated. Later studies have shown that Griswold was right.

In 1895 he instituted a study of the underground temperature of the rock walls of the Calumet and Hecla mine, and found that the increase is only 1° F. for every 223.7 feet as we descend. His deepest temperature observation was 4,580 feet beneath the surface of the ground.

He had now seen all of the coral reefs of the Atlantic and turned his attention to the exploration of the Pacific. In April and May, 1896, he cruised along the Great Barrier Reef of Australia in the little steamer *Croydon,* which he chartered from the Australian United Steam Navigation Company, Captain W. C. Thomson being in command.

The voyage began at Brisbane in April, extended northward to the Hope Islands and ended at Cooktown in May. Unfortunately, he had come to Australia in the height of the trade-wind season, and the almost constant gale greatly hindered the work of his expedition. Indeed, during more than a month of cruising he could spend only three days on the outer reefs, and the dredging and study of marine life which he had hoped to carry out were practically abandoned.

He concluded that the many islands and submerged flats off the Queensland coast were once connected with the mainland, but have been separated from the continent by erosion and denudation. After the formation of these flats and islands corals grew upon them. The recent reefs have been elevated at least ten feet, and do not owe their contours to subsidence, yet they form true atolls. The inner channels of the Barrier Reef are maintained free of corals by the great amount of silt held in suspension in the water or deposited to form a blue mud over the bottom. Thus there appears to be nothing in the Great Barrier Reef region to lend support to Darwin's theory of coral reefs.

A tangible result of this expedition was the enriching of the museum at Cambridge by a great collection of Barrier Reef corals gathered under Alexander Agassiz's auspices by H. A. Ward.

His experience in Australia taught him to avoid the trade-wind season and henceforth his expeditions to coral seas were timed so that he cruised among the reefs in the late spring and early summer months when the trades have died out into the long hot days of calm which precede the coming of the hurricanes. This interval when the torrid sea is sleeping gave him the opportunity to land on many a jagged shore that defied approach at other seasons. He then could wade through the still waters over the coral reefs, and unravel at his will the secrets of the atolls, composed as they are of wave-tossed fragments that once were shells of mollusks or skeletons of creatures of the reefs. His overmastering interest carried him to the shores of hundreds of these distant atolls where the cocoa-palm, the *Pandanus* and the fishes of the reef afford the only sustenance for man, where there are no hills or streams and the land is only a narrow strip across which one hears constantly the roar of heavy breakers.

These years of cruising accentuated his already predominant self-reliance, for the commander of a marine expedition must needs be an autocrat by profession. He was accustomed to command and to be obeyed and his relation to the Harvard Museum during these later years was in miniature similar to that of Bismarck to the German empire. Indeed, there was a strange physical and mental resemblance between Alexander Agassiz and Bismarck. Fearless, resolute, quick to anger, definitely purposeful and full of resource, they were closely akin in character, and indeed there seemed much in common between

the two, for during the course of his long and honored life Alexander Agassiz had been granted many interviews with exalted personages, but his meeting with Bismarck was the only one to which he delighted to refer. Alexander Agassiz was a colossal leader of great enterprises, fully as much as he was a man of science.

The cold winters of Cambridge were intolerable to him, and each year from 1875 until the close of his life he sought a more genial climate. Upon these pleasure excursions he visited Mexico, Central America, the West Indies, India, Ceylon, Japan, the readily accessible parts of Africa, and every country in Europe. He never went far into the arctic regions, although he saw the midnight sun at North Cape and visited the Aleutian Islands. Upon all excursions of the last twenty years of his life his constant companion and friend was his son Maximilian.

In 1896 in collaboration with Dr. W. McM. Woodworth he published a paper upon the variations of 3,917 specimens of the medusa *Obelia* (*Eucope*), in which the authors show that aberrant specimens of *Obelia* are very common. This paper is illustrated by interesting photographs made from life by Dr. Woodworth. This is one of the last of the studies published by him from his Newport laboratory, the latest one being in 1898 upon the scyphomedusa *Dactylometra.*

From November, 1897, to January, 1898, he cruised among the Fiji Islands in the little steamer *Yaralla,* chartered from the Australian United Steam Navigation Company and under the command of Captain W. C. Thomson.

Dana had stated that the coral reefs of the Fiji Islands were typical examples of the theory of Darwin, and Agassiz was greatly surprised therefore to find the clearest evidence of elevation, for in some places, as at Vatu Vara Island, the late Tertiary limestones are lifted more than 1,000 feet above the sea. This great elevation, which is so evident in numerous places among the Fiji Islands, probably took place in later Tertiary times and since then the islands have been greatly eroded and reduced in size, deep valleys being cut into their mountain slopes and many of the islands having been washed away by the tropical rains, leaving only a submerged flat. The coral reefs that grew around the shore line of the islands still remain after the islands have washed away, and thus the living reefs now mark the contours of the islands as they were. The currents flowing in and out of openings in the reef-rim have deepened the lagoons, but nevertheless there are many coral heads growing in the lagoon of every coral atoll.

He saw that the coral reefs which grew around a volcanic mountain remain after the mountain has washed away, and thus an atoll is formed without the agency of subsidence. In other cases, as at Fulangia, there was once an elevated coral limestone island lifted above sea-level. Then

rain water and atmospheric erosion leached out depressions in its central parts and finally the sea entered, forming a lagoon surrounded by a ring of detached islets of elevated limestone. In other cases the crater rims have washed away and a ring of coral reefs now marks the site of the old volcanic ridge.

According to Agassiz the coral reefs of to-day, in the Fiji Islands, form only a crust of moderate thickness upon a base of old limestone or volcanic rock. The present corals form only fringing reefs along the shores, and the contours of the atolls and barrier reefs are thus due to causes which acted at the time when the islands were elevated late in the Tertiary period.

In so great an archipelago as that of the Fijis with more than 270 islands there must be many details of reef formation the elucidation of which requires more prolonged study than Agassiz was enabled to devote to them in his visit of less than three months; for example, he was puzzled to explain the great thickness—1,000 feet and more—of the elevated limestones; for reef-corals do not grow at depths greater than about 120 feet. Could these enormous accumulations have been formed by coral reefs during a period of slow subsidence, as Darwin had assumed, or were they merely the talus of broken fragments which had rolled down the sea-ward sides from the outer edges of the reef, or were they formed by a slow accumulation of limestone fragments and shells of marine creatures other than corals which had lived upon the bottom more than a thousand feet beneath the surface and gradually built up a vast mass of limestone, as was the case with the great submerged Pourtalès Plateau off the Florida coast? He had in mind the fact that even in the richest coral reef regions the masses of broken shells and fragments of calcareous plants are commonly vastly greater than the bulk of the corals, for the corals grow only here and there over the limestone flats, and flourish luxuriantly only on their seaward slopes.

Were such a reef to form during a long period of slow subsidence, and then become elevated above the sea we should find only an occasional coral here and there imbedded in a great mass of limestone. This is the appearance presented by some of these elevated limestone cliffs of the Pacific islands, while others appear to be walls of non-coral-bearing limestone capped above with a crust of corals. In many cases, however, the corals they once contained have disappeared and been replaced by calcite or dolomite. These elevated limestones soon become very hard when exposed to the atmosphere, for they become coated by a dense veneer which rings with a clinker-like sound when struck with a hammer. One sometimes finds shells of the giant clam, *Tridacna,* imbedded in this hard limestone and elevated above the sea, and yet the nacre of the shell is still white and polished, thus proving that the rock

was elevated only recently, and certainly not longer ago than in late Tertiary times.

Altogether the most interesting problem raised by Alexander Agassiz's researches in the Pacific is the question of the relation between these elevated tertiary reefs and the growing coral reefs of to-day, and it still remains unsolved, despite the careful studies made by Mr. E. C. Andrews, whom Alexander Agassiz sent to the Fiji Islands especially to study this problem, for Andrews's investigation has merely served to show that the subject is very complex and can not be solved until prolonged study of certain favorable localities has been completed.

From August, 1899, until March, 1900, Alexander Agassiz had for the second time the scientific direction of the *Albatross*. Commander Jefferson F. Moser, U.S.N., was in command and the cruise began at San Francisco and extended across the tropical regions of the Pacific to the Ladrone Islands and thence northward to Japan. On this great cruise the *Albatross* visited the Marquesas, Paumotos, Society, Cook, Nieue, Tonga, Fiji, Ellice, Gilbert, Marshall, Caroline and Ladrone Islands, steaming many thousands of miles in and out among the atolls.

From San Francisco the vessel steamed 4,000 miles straight to the Marquesas, making many soundings and trawl hauls which led to the discovery that there is here a great basin between 2,500 and 3,000 fathoms deep, the bottom of which is covered with manganese nodules and the teeth of extinct sharks. It was an impressive sight to see the great trawl bring up tons of the manganese nodules looking like gritty brown potatoes, and all nearly as cold as ice, for the temperature of the deep floor of the ocean here was less than 3° F. above freezing. Despite the heat of the tropic sun beating upon our deck our hands stung with the cold as we felt among the mass of nodules and cracked them open to discover the enclosed nucleus of pumice, the encrusted ear-bone of an extinct whale or a shark's tooth imbedded in the soft brown rock. Some of these shark's teeth were so large that the shark itself was probably more than a hundred feet long. A deep submarine area far greater than that of the United States is covered thickly by these manganese nodules and sharks' teeth, and Alexander Agassiz named it the "Moser Deep," in honor of the commander of the *Albatross*.

Very little animal life was found, either floating in the sea or on the bottom, over this vast desert of manganese nodules.

The chief result of this expedition was the discovery that a widespread elevation of the Pacific islands occurred in late Tertiary times. The Hawaiian, Paumotos, Society, Cook, Nieue, Tonga, Fiji, Ladrone and Caroline Islands all show elevated coral or limestone reefs, but there are no visible indications of elevation in the Marshall or Gilbert Islands where the underlying rock is not lifted above the sea. Makatea in the Paumotos may have been an atoll which was elevated about 230

feet above the sea and with a lagoon-basin in the center sunken about 70 feet below the encircling ridge. It is possible, however, that this central concavity may have been formed by solution after the island was raised above the sea, and that the island was not originally an atoll.

The lagoons of the Pacific atolls were found to be usually from 13 to 20 fathoms deep, and to be quite thickly studded with submerged rocks consisting of Tertiary limestone encrusted with modern corals.

The atoll contours are due to a coordination of complex conditions, erosion, currents, silt, etc., which determine the place and rates of growth of the corals; and not to subsidence, as was postulated by Darwin.

The modern coral reefs are, according to Agassiz, distinct from the tertiary limestones, and form a mere crust upon a base of lava or of old limestone.

A notable act of the expedition was the bringing up of the deepest trawl haul ever made, this being from a depth of 4,173 fathoms, seventy-five miles east of Tonga Tabu. Siliceous sponges were found here under an ocean almost as deep as the crests of the Himalayas are high.

In Bora Bora, of the Society group, he found a broken ring of sandy coral islets covered with cocoa palms, and encircling the shallow waters of the lagoon, out of the center of which there arises the towering mass of the basaltic cliffs of the island. The sight of this old volcano, now sleeping and encircled by its palm-crowned atoll ring, so impressed Alexander Agassiz that he employed Mr. G. W. Curtis to make a survey, and to construct a detailed model of the island for the museum at Harvard.

As one goes westward over the tropical Pacific the coral heads upon the reefs become larger and larger, those of the Paumotos being small and stunted, while those of the Great Barrier reef of Australia are the largest in the world.

Alexander Agassiz had now seen nearly all of the coral islands of the Pacific, and he at once turned his attention to the Indian Ocean, cruising among the atolls of the Maldive Islands from December, 1901, to January, 1902. For this purpose he chartered the steamer *Amra* from the British India Steam Navigation Company, William Pigott, R.N.R., in command. He steamed more than 1,600 miles among the islands, making more than eighty soundings. Mr. J. Stanley Gardiner, M.A., had only recently explored the Maldives, and his account of their mode of formation was published before that of Agassiz. Both Gardiner and Agassiz agree that there is evidence of recent elevation in the Maldives, and that conditions which are operating at the present day are determining the shape of the atolls. Shifting sand-bars play a considerable rôle in determining the contours of the atolls, some of them being mainly rings of sand-bars enclosing a lagoon, as in the Gilbert

Islands. No elevated tertiary limestones were seen, but the modern coral reef is in places now above the sea. In essential respects Gardiner and Agassiz are in accord and both decide that Darwin's theory is not applicable to the Maldives. They differ, however, in the conception of a "perfect atoll," and in their opinions of some of the causes which have led to the deepening of the lagoons, but the discussion of these matters would be unprofitable in this place.

Dr. Henry B. Bigelow was an assistant upon this expedition and wrote a report upon the Medusæ.

After his return from this expedition Alexander Agassiz was not suffered to remain long at rest, for once again, for the third and last time, he was given charge of the *Albatross*. The *Albatross* left San Francisco on October 6, 1904, and steamed to Panama. Thence to the Galapagos Islands, then to Aguja Point and Callao on the Peruvian coast, and then to Easter Island, from which she returned to the Galapagos, only to again venture out over the Pacific to Manga Reva, then back to Acapulco, and home to San Diego, where she arrived in March, 1905. Lieutenant Commander L. M. Garrett, U.S.N., was in command, and they crossed and recrossed the Humboldt current four times, cruising more than 13,000 miles, making 160 deep-sea soundings and 280 pelagic hauls. The expedition ranged over the largest uninterrupted area of ocean in the world. Professor C. A. Kofoid collected the protozoa and Dr. Henry B. Bigelow the medusæ, while the coral reefs, oceanography and echinoderms were studied by Alexander Agassiz.

Interesting photographs of the great stone images of Easter Island were obtained, and it was found that Manga Reva is a barrier reef island with an eroded volcanic center.

A remarkable result of the expedition is the discovery that the cold Humboldt current, which flows northward along the western coast of South America from the Antarctic to the equator, is a great bearer of pelagic life teeming with medusæ, salpæ and all manner of floating creatures both on the surface and in its depths; but in the outer Pacific beyond the western edge of this great current we find a vast area almost barren of life. Also the bottom under the Humboldt current is crowded with organisms, whereas there is a sparsely inhabited submarine desert to the westward of the western edge of the current. The effect of this current upon the distribution of pelagic life is most clearly described by Henry B. Bigelow in his account of the medusæ of this expedition.

This was Alexander Agassiz's last great scientific expedition, although in 1908 he made a brief visit to the Florida Reef, and from February until March, 1907, he cruised through the West Indies from Porto Rico to Grenada in the chartered yacht *Virginia*. Dr. Henry B. Bigelow was his scientific assistant, and many pelagic hauls were made, but the region was found to be almost barren of floating life. This is

an extraordinary fact, and it applies at present to the whole vast region of the West Indies, thus from 1877 until 1898 the region of the Tortugas, Florida, was noted for the variety and richness of its floating life, but since that time the pelagic animals have become rarer year by year until at present the region is almost a desert sea.

In August, 1907, he presided over the meeting of the seventh International Zoological Congress at Boston, and his presidential address is an account of the publications which had resulted from his many expeditions, and the reports of those to whom he had sent collections. These include the most noted specialists in all of the highly civilized countries of the world.

In the winter and early spring of 1908 he visited the equatorial lake regions of Central Africa, the expedition being mainly a pleasure trip.

Between 1907 and 1909 he published five papers upon Pacific echini with Dr. Hubert Lyman Clark as joint author, and other papers of this series are still to appear.

In common with all students of pure science in our country, Alexander Agassiz was far more highly appreciated abroad than he was at home, for in our country practical applications and the invention of mechanical devices compass nearly all that the general public cares for science, and indeed our republic is without means to confer honors upon its scientific men. Thus while he was an honorary member of all of the great scientific societies of Europe and had been recognized officially by the republic of France and the German emperor, only one American university (his alma mater) conferred upon him an honorary degree.

In 1898 he was made an Officer of the Legion of Honor of France and in 1902 a Knight of the Order of Merit of Prussia. He was a foreign associate of the Academy of Science of the Institute of France, the only American associates of that time being Agassiz and Newcomb. He was foreign honorary fellow of the Royal Society of Edinburgh, foreign member of the Royal, Linnean and Zoological societies of London, honorary member of the Royal Microscopical Society of London, and honorary member of the academies of Berlin, Prague, Göttingen, Leipzig, Munich, Manchester, Vienna, Upsala, Stockholm, Copenhagen, Liège, Moscow, Rome, Bologna, Geneva, Mexico, etc.

He received the honorary degree of LL.D. from Harvard in 1885 and from St. Andrews, Scotland, in 1901, Ph.D. from Bologna in 1888 and honorary Sc.D. from Cambridge in 1887.

In 1878 he was awarded the Prix Serres by the Paris Academy, being the first foreigner to be thus honored, and in 1909 he received the Victoria research medal of the Royal Society of London.

After the publication of the results of the Maldive and eastern Pacific expeditions, one great and final task lay before him. This was to present a summary of the results of his twenty-five years of study

of the coral reefs of the world. Five years would have been required for the preparation of this crowning work which would have borne the same relation to his coral-reef studies what his "Three Cruises of the Blake" did to his early deep-sea work—an epitome of the whole subject. For eighty-two years the Agassiz father and son had been active leaders in science, and he hoped for five more years of productivity.

But this was not to be. He had for several years been suffering from an impairment of the circulation, and had retreated for rest and recreation to the genial climate of Egypt and southern Europe.

He was returning from England in the steamship *Adriatic* and never did he appear to be in happier mood than upon the night of the twenty-sixth of March, 1910, but on the morning of the twenty-seventh he failed to appear, and when his son Maximilian entered his father's cabin it was seen that he had fallen into his last long sleep. Many a guarded secret had the ocean revealed to him, and it was fitting that far from the sight of land with only the waves around there came to him the mystery of death.

When I was young and struggling, his hand befriended me, and his great mind gave direction to the thought of the life I have led, and I think upon his spirit with gratitude and reverence, for he was my master in science.

RECENT DEVELOPMENTS IN PHYSICAL SCIENCE

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THERE has been a revolution in the physical science of to-day compared with the physical science of twenty-five years ago. In the first place there has been a revolution in the methods of teaching science. The physics laboratory of the University of Berlin was founded in 1863, the Cavendish Laboratory, of Cambridge, in 1874. In 1871 Professor Trowbridge, of Harvard, was obliged to borrow certain electrical measuring instruments, as the university had none of its own. It is not surprising, then, that twenty-five years ago there were in the United States very few physics laboratories worthy of the name. Physics teaching in college and high school was chiefly from the text-book. To-day a college which would offer work in physics without a laboratory would be considered a joke; and in order to be commissioned to enter its students into the freshman class of a college, a high school must have a certain minimum of laboratory equipment and the physics teacher must devote a part of his time to laboratory instruction.

In the second place there has been a complete change in the attitude of men of affairs toward the physics professor and his students. No longer do they consider us theoretical, and therefore impractical. No longer do they look with distrust or contempt on laboratory methods and data. No longer do they hold that apprenticeship and experience are sufficient for their needs. To-day the large industrial concerns are establishing laboratories of their own and employing in them the best trained men they can command.

In the third place there has been a revolution in some of our physical theories. By the term revolution I do not mean a destructive upheaval in which the work of the past has been repudiated and destroyed and a new order of things established. I mean that some of our ideas have undergone such a complete and rapid change that what some might term an evolution is really a revolution. Indeed, we have had two revolutionary periods within the past twenty-five years.

The first came in 1887 with the epoch-making researches of Heinrich Hertz. Faraday had given us his theory of lines of force and the mathematicians had attacked it. Young and Fresnel had given us the undulatory theory of light and Laplace and Poisson had "befuddled us with their objections." Ampère had given a theory of magnetism, but Poisson and Weber had given two others. To explain an electric charge we could resort to the one-fluid theory, the two-fluid theory, the potential theory, the energy theory, the ether strain theory. Maxwell

had written a treatise on electricity which few could read and no one could fully understand. A distinguished French physicist said he understood everything in Maxwell's book except what was meant by a body charged with electricity. Maxwell had given us but a vague idea of electric displacements and displacement currents, because his ideas were bound up in equations without experimental verification, or even illustration.

Then came Hertz's researches which confirmed the fundamental hypotheses of the Faraday Maxwell theory and "annexed to the domain of electricity the territory of light and radiant heat." "Many thinkers," said Lord Kelvin,[1] "have helped to build up the nineteenth-century school of *plenum,* one ether for light, heat, electricity and magnetism; and Hertz's electrical papers, given to the world in the last decade of the century, will be a permanent monument of the splendid consummation now realized." Some one has said that Hertz enthroned Maxwell in every chair of physics in Europe and America.

It appears that many of the ancient philosophers had a shadowy idea of a medium in space, which they personified, and called "Æther." According to Heriod Æther was the son of Erebus and Night and the brother of Day. The Orphic hymns speak of Æther as the soul of the world, the animator of all things, the principle of life. The children of Æther and Day were the objects about us, the heavens with all their stars, the land, the sea. Æther was the lightest and most active form of matter and Day had the power of converting it into heavier matter. Plato speaks of the æther as being a form of matter far purer and lighter than air, so light that "its weight can not be ascertained because distributed through infinite space."

During the fifteen years following the publication of Hertz's researches, it is probable that greater homage was paid to ether by modern physicists than was ever given it by the ancients. The ether was appealed to from every quarter. Light, radiant heat and electric waves were ether waves. An electric charge was an ether strain. An electric current was a phenomenon in the ether and not in the wire in which it appeared to flow. Magnetism and gravitation were phenomena of the ether. Matter itself became an aggregation of ether vortices. Ether and motion were expected to explain everything. Such terms as natural philosophy and physics were discarded by some of our text-book writers who adopted such titles as "matter, ether and motion"; "ether physics"; "ether dynamics"; "the mechanics of the ether." Physics was defined as the science of motion.

The classical mechanics of La Grange was built on what were con-
in his treatise on mechanics published in 1894, endeavored to eliminate
sidered fundamental concepts—mass, force, space and time. Hertz,

[1] Kelvin, introduction to Jones's translation of Hertz's "Electric Waves." Macmillan, 1893.

force and potential energy, and reduce a universe to ether movement. Space and time were not fundamental ideas, but, as Kant had said, mere subjective notions. We measure time by a change of space relation; that is, a movement of a star, of the earth, of a clock hand. "In a world void of all kind of movement there would not be seen the slightest sequence in the internal state of substance. Hence the abolition of the relation of substances to one another, carries with it the annihilation of sequence and of time." Thus everything was made to depend upon movement. The equations of motion became the chief instruments of physical research, and the criterion by which the results of experiments were interpreted. Galileo lost his professorship because he dared to dispute the authority of Aristotle. Daguerre was, for a time, placed in an asylum because he said he could take a picture on a tin plate. Galvani was ridiculed by his friends, and dubbed "the frog's dancing master." Franklin's paper on lightning conductors was considered foolish, and refused publication by the Royal Society. Fifteen years ago it would have been almost as disastrous for a physicist to question the authority of La Grange or Maxwell. Not only were the results of experiments subjected to mathematical analysis, the *direction* of scientific investigation was largely so determined. The question was first put to mechanics. If a positive answer was indicated the question was put to nature and the research went on. If the equations indicated a negative result the question was dropped and the research abandoned.

Physics was an *exact* science. Other sciences were not exact sciences because their theories and hypotheses could not be mathematically expressed—the relation between cause and effect was not expressible in algebraical symbols. Physics was an exact science whose fundamental principles had been discovered and its laws expressed by equations. All that remained to be done was to make more accurate measurements of physical quantities for use as coefficients and exponents.

Let me quote from the 1894 catalogue, and later catalogues, of one of the largest universities in the United States.

> While it is never safe to affirm that the future of physical science has no marvels in store . . ., it seems probable that most of the grand underlying principles have been firmly established and that further advances are to be sought chiefly in the rigorous application of these principles to all the phenomena which come under our notice. . . . An eminent scientist has remarked that the future truths of physical science are to be looked for in the sixth place of decimals.

The foregoing is a verbatim quotation from the introductory statement preceding the list of courses in physics offered at one of our great universities, written, I think, in 1894. "Underlying principles firmly established." "Future truths in sixth decimal place," 1894. Then came the discovery of Röntgen rays, 1895; Becquerel rays, 1896; Zee-

man effect, 1896; radium, 1898; atomic disintegration, the transformation of matter, the thermal effect of radioactivity, and intra-atomic energy, 1903. I am unable to locate the sixth decimal idea in recent catalogues.

J. J. Thomson likens the discovery of Röntgen rays to the discovery of gold in a sparsely populated country. Workers come in large numbers to seek the gold, many of them finding that "the country has other products, other charms, perhaps even more valuable than the gold itself."

The chief value of Röntgen's discovery was not that it furnished us a "new kind of light for the investigation of dark places," but in the fact that it led a host of workers to study vacuum-tube discharges—the discharge of electricity in gases and the effects of such discharges on matter itself. The old dusty Crookes tube was taken down from the far corner of the upper shelf and regarded with new interest. In a day it had ceased to be a forgotten, though curious, plaything, and had become a powerful instrument of research. It was before Röntgen's discovery that a well-known professor said to me that he considered it foolish for one to spend any part of his departmental appropriation for a vacuum; that when he paid out money he wanted something in return—not an empty space. And yet this man was familiar with the work of Faraday and of Crookes, both of whom with prophetic mind had foreseen and foretold. Let me quote from a lecture by Faraday, on the significant subject, "Radiant Matter." [2]

> I may now notice a peculiar progression in physical properties (of matter) accompanying changes of form, and which is perhaps sufficient to induce, in the inventive and sanguine philosopher, a considerable degree of belief in the association of the radiant form with the others in the set of changes I have mentioned.
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> As we ascend from the solid to the fluid and gaseous states physical properties diminish in number and variety, each state losing some of those which belong to the preceding state. . . . The varieties of density, hardness, opacity, color, elasticity and form, which render the number of solids and fluids almost infinite, are now supplied by a few slight variations in weight and some unimportant shades of color.
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> To those, therefore, who admit the radiant form of matter, no difficulty exists in the simplicity of the properties it possesses. . . . They point out the greater exertions which nature makes at each step of the change and think that, consistently, it ought to be greatest in the passage from the gaseous to the radiant form.

The lecture from which the foregoing is a quotation was delivered in 1816, when Faraday was but twenty-four years old.

Let me quote again, this time from a lecture by Sir William Crookes, delivered sixty years later, more than twenty years ago, on the same subject—"Radiant Matter."

[2] "Life and Letters of Faraday," Vol. I., p. 308.

In studying this fourth state of matter we seem at length to have within our grasp and obedient to our control the little indivisible particles which with good warrant are supposed to constitute the physical basis of the universe. We have seen that in some of its properties radiant matter is as material as this table, whilst in other properties it almost assumes the character of radiant energy. We have actually touched the borderland where matter and force seem to merge into one another, the shadowy realm between known and unknown, which for me has always had peculiar temptations. I venture to think that the greatest scientific problems of the future will find their solution in this borderland, and even beyond; here, it seems to me, lie ultimate realities, subtle, far-reaching, wonderful.

The developments of the last few years have demonstrated that no truer prophecy was ever uttered, and the prophet Crookes has lived to witness and to take a part in its fulfilment.

The importance of the present rejuvenation of physical science does not consist alone in the abundance of the harvest. There have been abundant harvests in the past. Consider the decade which closed one hundred years ago. In 1798 Rumford boiled water by friction. In 1799 Davy melted ice by friction in a vacuum and Laplace published his work on mechanics. In 1800 Volta constructed the Voltaic pile, Nicholson and Carlisle decomposed water, Davy discovered the properties of laughing gas and Herschel discovered dark heat rays. In 1801 Piazzi discovered the first asteroid, Ritter the chemical rays and Young the interference of light. In 1802 Wedgewood and Davy made sun pictures by the action of light on silver chloride, and Wollaston discovered dark lines in the sun's spectrum. In 1808 Malus discovered polarization by reflection, Gay Lussac the combination of gases by multiple volumes and Dalton the law of multiple proportions.

So great was the exhilaration and satisfaction produced by these discoveries that many scientists of that period appear to have become infected with something akin to the "sixth decimal" delusion. "Electricity," wrote the French scientist Haüy, "enriched by the labor of so many distinguished physicists seems to have reached the time when a science has no more important steps before it, and only leaves to those who cultivate it the hope of confirming the discoveries of their predecessors and of casting a brighter light on the truths revealed." A statement which was almost immediately followed by the discoveries of Oersted, Ampère, Seebeck and Faraday. A statement which has been followed by the telegraph, the telephone, the dynamo, the motor, the electric light, the electric railway, the Röntgen rays and the wireless telegraph and telephone.

If any one to-day is disposed to criticize the men of science of other times because of their limited view, their complacent opinions and their intolerance of all that did not agree with theories they considered established, let him first read and ponder over what One spake about motes and beams.

The real significance of recent developments is in the fact that they change—in a way revolutionize—some of our ideas of things. And here let me say that proved facts and proposed theories should not be confused. A theory is simply a working hypothesis, invented for the purpose of explaining facts, to be discarded when facts are discovered with which the theory is not in harmony. A theory may explain many facts, it may be generally accepted, it may have survived for generations, and be false. The phlogiston theory, the corpuscular theory, are two examples. Shall we say that the theory of the indestructibility of matter, and of the conservation of energy, are two others?

The usual chemistry text-book would have us believe in the indestructibility of matter because the chemist can change the form of matter almost at will, and in all the chemical reactions there is no loss of weight. In replying to this argument I wish to make three points:

1. The balance, notwithstanding the statement of text-books, compares weights and not masses, and it is only because weight is assumed to be proportional to mass that we say we determine mass by the balance. What we really compare is the gravitational force which the earth exerts on two masses, and we have no *a priori* right to assume that this gravitational force is absolutely independent of the state or molecular arrangement of the attracted body. Why, for instance, should we expect an absolutely uniform field of force about a crystal when that same crystal will, if placed in a proper solution, continue to grow symmetrically, and perhaps replace a broken-off corner before beginning its growth?

It is conceivable that there should be a loss of weight in chemical reactions and yet no destruction of matter. It is possible that mass and weight are not strictly proportional. If J. J. Thomson were not disposed to question the equation $w = m.g.$ he would not have experimented with a pendulum of radium, and he would not now be experimenting with a pendulum of uranium oxide.

2. In the second place there *is* an apparent change of weight in chemical reactions as has been shown by several experimenters—notably by Landolt,[3] who found a loss in forty-two out of fifty-four cases. The chemical reactions were brought about in sealed glass tubes which generally weighed less after the reactions than they weighed before. Later[4] it was found that some of these losses might be attributed to temperature and volume changes. Whatever the testimony of the balance may have been, some of the reactions must have been accompanied by a loss of weight, for it has been proved by chemical means that such reactions are frequently attended by the escape of something through the walls of the glass tubes.[5] This loss is readily explained by the disintegration

[3] Landolt, *Preuss. Akad. Wiss. Berlin, Sitz. Ber.*, 8, pp. 266–298, 1906.

[4] Landolt, *Preuss. Akad. Wiss. Berlin, Sitz. Ber.*, 16, pp. 354–387, 1908.

[5] C. Zenghelis, *Zeitschr. Phys. Chem.*, 65, 3, pp. 341–358, January 5, 1909.

theory. If one wishes to explain it by assuming the diffusion of ordinary gases through the glass walls of the tube, he must explain the fact that in many cases it was the heavy and least volatile substances that escaped fastest.

3. In the third place the element of time has been overlooked. Matter may be disintegrating, but at such a slow rate that in the limited time over which experiments have been extended the balance has failed to detect the change. As far as our experience goes the time of rotation of the earth is constant; but we know that it can not be absolutely constant. The moon has slowed down until it takes a month to make one turn. To an ephemeral insect almost everything would appear to be eternal.

With due respect for the balance and the wonderful work it has enabled chemists to do, it must be admitted that it is, comparatively, a very crude instrument. Let me prove it.

Suppose we fix the limit of sensibility of the chemical balance at one one-thousandth of a milligram. Our books on chemistry tell us that 1 c.c. of gas, say hydrogen, at ordinary pressure contains 4×10^{19} molecules. The density of H being 896×10^{-7}, then 1 gm. of H would consist of $(4 \times 10^{19}) \div (896 \times 10^{-7})$ molecules. Taking 112 as the ratio of the molecular weights of radium and H, then 1 gm. of radium would consist of $[(4 \times 10^{19}) \div (896 \times 10^{-7})] \div 112 = 4 \times 10^{22}$ molecules. Therefore .001 mgm. of radium would consist of 4×10^{16} molecules, and this would be the smallest possible number that our most sensitive balance could detect. If the gram of radium were disintegrating and its molecules escaping at the rate of a million per second it would require 4×10^{10} seconds $= 463{,}000$ days $= 1{,}270$ years for that gram of radium to lose in weight only the one-thousandth part of one milligram, all the while its molecules trooping away at the rate of a million per second.

The population of the earth is about 1,500 millions. The smallest number of molecules a balance will detect is 4×10^{16}, or about 26,600,000 times the population of the earth. We wonder if Mars is inhabited. If a Martian were to come to the earth to make an experiment to determine whether or not the earth is populated and he had no better instrument "for the detection of the existence of a man" than is the balance for a molecule, he would be obliged to go back and report the earth uninhabited. In fact his instrument for the man test would need to be 26,600,000 times as sensitive as the balance to give him even a hint of the probability of an earth population.

Thomson says that the smallest quantity of unelectrified matter ever detected is probably neon, and this was discovered by the spectroscope —not the balance. But the number of molecules of neon required to give a spectroscopic effect is about ten million million, or about 7,000 times the population of the earth. It has been shown that the presence

of a single charged atom can be detected by electrical means. Thus the electroscope is millions of millions of times as sensitive as the spectroscope, which is itself in many cases far more sensitive than the balance. This explains, in part, why radium was discovered by physicists, and why physicists have been most active in all the work which has had to do with the theories of electricity and matter. If chemists wish to compete with physicists in this field of investigation they must adopt physical methods and apparatus, or devise some which shall be far more sensitive than the balance or spectroscope. Further, some of the great chemists of the world need to awake to the fact that there is something doing and that they are not doing it. The conservatism and indifference of some of them are surprising. But a few months ago one of them expressed the following sentiments in a paper read before the Chemical Section of the British Association.[6] ". . . those who feel that the electron is possibly [note the possibly] but a figment of the imagination will remain satisfied with a symbolic system which has served us so long and so well as a means of giving expression to facts which we do not pretend to explain. . . . Until the credentials of the electron are placed on a higher plane of practical politics, until they are placed on a practical plane, we may well rest content with our present condition and admit frankly that our knowledge is insufficient to enable us even to venture on an explanation of valency." Think of it! We, the chemists, " remain content," in this day when, as the Hon. A. J. Balfour has said, the attempt to unify physical science and nature[7] " excites feelings of the most acute intellectual gratification. The satisfaction it gives is almost esthetic in its intensity and quality. We feel the same sort of pleasurable shock as when from the crest of some melancholy pass we first see far below the sudden glory of plain, river and mountain." " Rest content!" No wonder the Nobel prize in chemistry was awarded to Rutherford, a physicist.

As to the second principle, the conservation of energy, some have had misgivings. It was Kelvin, I believe, who said that radium placed the first question mark after this great principle. Many have refused to believe in the electron and disintegration theories because they saw, or thought they saw, in these theories a contradiction of the principle of energy conservation. Personally I do not see that there are necessarily any contradictions. But even if there were and we were therefore justified in rejecting the theories proposed to explain the facts, we certainly should not be justified in rejecting the facts themselves.

In this connection I am reminded of the story of a lawyer whose client was placed in jail for some very trivial offence. When the lawyer learned the nature of the charge he said to his client: " My

[6] *Scientific American Supplement*, 63, No. 1761, p. 210, October 2, 1909.

[7] " Reflections Suggested by the New Theory of Matter," presidential address, British Association for the Advancement of Science, 1904. *Science*, 20, No. 504, pp. 257–266, August 26, 1904.

friend, they can not put you in jail on such a charge as that." "Yes, but they have," replied the prisoner.

When our physicist says that radium can not remain at a higher temperature than its surroundings and continue to radiate heat, as that would be contrary to the second law of thermodynamics, the answer is —yes, but it does.

When he says that it can not continue to radiate energy without receiving energy from some other body, as that would be contrary to the principle of the conservation of energy, the answer is—yes, but it does it.

When some one says that helium or carbon dioxide can not appear in sealed tubes which contained no trace of these substances to begin with, the answer is—yes, but they do.

Let us suppose that we have a mass of gun powder and that it is possible to, and we do, cause it to explode, one grain at a time, each grain firing its neighbor—as in the fuse of a fire cracker. The temperature of the mass of gunpowder will be higher than its surroundings and it will give off heat and other forms of energy, and continue to do so so long as the powder lasts. No one would think of calling this an exception to the law of the conservation of energy, or the second law of thermodynamics. The source of the energy is the atomic potential energy of the powder itself.

Let us suppose that we have a sphere with frictionless surface rotating at an enormous speed. Suppose that particles of matter are thrown off at frequent intervals. These particles, on account of their high speed, possess considerable energy. Thus the sphere continues to give off energy without receiving any, as long as any mass remains. The source of the energy is the kinetic energy stored in the sphere at the outset, of which energy we are conscious only when we have some method of detecting and slowing down the projected particles.

Thus the energy radiated by radium might be stored within the radium atom as potential energy and liberated by a sort of atomic—or sub-atomic explosion. Or it might be stored as kinetic energy—of revolving electrons and liberated gradually as these electrons escape from their orbits. It might be stored in both forms. In any case it is intra-atomic energy because stored *within* the atom itself, and liberated only by atomic change—disintegration. In neither case would there be a violation of the principle of the conservation of energy or of the second law of thermodynamics. Sooner or later all the energy will have been radiated. The fact that the supply is destined to last so long is what appeals to us as wonderful. And so it is. The world is full of wonderful things to any one who pauses long enough to think.

In this paper I have endeavored to give a general notion of the trend of physical science rather than an enumeration and description of dis-

coveries and theories. I might say, however, that there are strong reasons for believing in the molecular structure of electricity, the electrical nature of matter and the dependence of mass upon velocity. The theories of radioactivity and disintegration of matter are fairly well established. According to Ramsay, one of the most eminent chemists in the world, "we are on the brink of discovering the synthesis of atoms, which may lead to the discovery of the ordinary elements." Perhaps the dream of the alchemist is about to be realized. Certain it is that we are face to face with energies of which no one even dreamed a few years ago. Whether we call this energy intra-atomic, sub-atomic, inter-elemental, or some other name, we know certainly that it exists, and that it exists in quantities far beyond the power of man's mind to comprehend. Man dares to hope, some day, somewhere, somehow, to discover the means of unlocking this infinite storehouse. If this discovery is ever made, all the others which man has ever made will pale into insignificance beside it.

Lodge says of the one-pound shot and the one-hundred-pound shot which Galileo dropped from the top of the Leaning Tower, that "their simultaneous clang as they struck the ground together sounded the death knell of the old system of philosophy and heralded the birth of the new." The age of reverence for authority had passed away and the day of experimental investigation had dawned.

In a sense the discoveries of the past few years have resulted in a similar revolution. The revival of the experimental method has been complete. Accepted theories are being put to the test. What we have long regarded as proved facts are being questioned and, in many cases, challenged. There is no field of investigation which has not been cultivated anew.

In closing, I wish to quote from the presidential address of J. J. Thomson[8] before the British Association at its last meeting.

> The new discoveries made in physics the last few years, and the ideas and potentialities suggested by them, have had an effect upon the workers in that subject akin to that produced in literature by the Renaissance. Enthusiasm has been quickened, and there is a hopeful, youthful, perhaps exuberant, spirit abroad which leads men to make with confidence experiments which would have been thought fantastic twenty years ago. It has quite dispelled the pessimistic feeling not uncommon at that time, that all the interesting things had been discovered, and all that was left was to alter a decimal or two in some physical constant. There never was any justification for this feeling, there never were any signs of an approach to finality in science. The sum of knowledge is, at present at any rate, a diverging, not a converging series. As we conquer peak after peak we see regions in front of us full of interest and beauty, but we do not see our goal, we do not see the horizon; in the distance tower still higher peaks, which will yield to those who ascend them, still wider prospects, and deepen the feeling, whose truth is emphasized by every advance in science, that "Great are the Works of the Lord."

[8] *Scientific American Supplement*, 63, Nos. 1757 and 1758, pp. 154, 155 and 174–176, September 4 and 11, 1909.

MATHEMATICAL DEFINITIONS IN TEXT-BOOKS AND DICTIONARIES

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THE word definition is defined as "fixing the bounds of," or "determining the precise signification of." It may be distinguished from the word description by saying that the latter merely makes its object known by words or signs, very often by some non-essential quality, as a lady by her dress. Young, in his "Teaching of Mathematics," says a definition is simply an agreement making clear the precise meaning of the word defined. As mathematics is an exact science, its definitions are important and play a significant part in the development of the subject. Formerly the tendency was to give a large number of definitions at the beginning of a study, but latterly only essential ones are furnished, and others are introduced as needed.

The distinction between definitions, axioms and postulates is often not clear, though it would appear that there should be definite boundaries between them. Doubtless so far as their etymological meanings go the words postulate and axiom could be used interchangeably. A few late geometries class axioms and postulates together and call them all postulates. German texts usually avoid the use of these terms altogether. To the writer the distinction between axiom and postulate in Euclid is valuable and should be retained. Fortunately, most American authors follow Euclid in regarding the postulates as the fundamental propositions of constructions, one the straight-line postulate, and the other the circle postulate. Similarly, some writers do not distinguish clearly between axioms and definitions. For instance, it is usually given as an axiom that quantities that can be made to coincide are equal. This, on the face of things, simply defines the meaning of the term equal. Again, some writers following the lead of the popular French geometer, Legendre, define a straight line as the shortest distance between two points, whereas Euclid gives this property as an axiom. This test for a straight line implies measurement, and hence the idea of measurement of lines would have to be developed before a straight line could be defined. Evidently Euclid's view of the matter is much preferable to Legendre's.

The definitions of the fundamental concepts by different authors should amount to the same, however differently they are expressed. But it turns out that definitions apparently meaning the same thing are really very different. Thus some authors have defined parallel

lines as lines that are everywhere the same distance apart (Webster); others that they are lines in the same plane that will not meet however far produced; and others still that they are lines having the same direction. In one sense these definitions mean the same thing, but in the development of geometry they give rise to very different series of propositions.

Perhaps the most far-reaching of differences in definitions are found in those for parallel lines. Wentworth, following Chauvenet, says parallel lines are those having the same direction throughout their whole extent. This definition is very objectionable for two reasons; first, because the meaning of the word direction is ambiguous, the word being used to signify either one way or the exactly opposite, or in the sense of the angle a line makes with a standard line; second, because the idea of direction in the sense intended is difficult to explain. The Century Dictionary gives this definition: "The direction of point *A* from point *B* is or is not the same as that of another point *C* from point *D*, according as a straight line drawn from *B* to *A* and continued to infinity would or would not cut the celestial sphere at the same point as the straight line from *D* to *C* continued to infinity." Chauvenet and Wentworth thought they had found a way to simplify the definition of parallelism. It is clear from the preceding that what they did was to slur over a very complex concept. As a matter of fact the use of the word direction in trying to define parallels was not new. Thus Dr. Johnson defines parallels "as lines extended in the same direction, and preserving always the same distance." The definition used by Euclid, viz., lines in the same plane that will not meet, however far produced, is practically the best, and has the merit of preparing the student for the non-Euclidean geometry.

Not only have parallel lines been defined differently by different authors, but other important terms have met the same fate. The concept angle has been presented in three or four ways: (1) As the figure formed by two lines meeting, which is essentially a description, not a definition. (2) As the difference in direction of two lines. (3) As the inclination of one line to another. (4) As the amount of divergence of two lines that meet. The objection to the first is that it does not call attention to an angle as a magnitude, but rather as a shape. A recent author gives this definition and then asks on the next line whether increasing the lengths of the lines would increase the size of the angle? Of course it would if the pupil judged by the definition given. The use of the term *direction* to define angle is as objectionable as for parallels. The third definition, Euclid's, is better than the others, but not as clear as it might be on account of the meaning of the word inclination. Thus we are led to the fourth definition, which is objectionable chiefly on the ground that, following the usual custom in English,

a Latin word is introduced to possess a technical signification, which has little or no meaning for most young persons. The word divergence should be explained etymologically in the definition as *turning apart.*

Some writers introduce the simple geometrical concepts without attempting to give any definition of them, these concepts being thought of as fundamental, and not needing or capable of definition, just as certain truths are taken as axiomatic. Thus Hilbert, in his "Foundations of Geometry," so regards the concepts of point, straight line, plane and angle. Among his axioms he includes one which says a straight line is "determined" when two of its points are given, and another which says a plane is determined when three of its points are given, the (virtual) definition for a plane thus corresponding to that for a straight line. Nearly all American authors say a plane is a surface such that if any two of its points be joined by a straight line, the line will lie wholly in the surface. To be consistent they should define a straight line as Euclid does, viz., as a line that lies evenly between any two of its points. Thus a plane surface is tested by laying a straight edge on it, and a straight line is tested by sighting between its points to see if they are in the line of sight.

There are four principal definitions for straight line, all different in character. Three of these have already been referred to. One of them, Legendre's (that a straight line is the shortest distance between two points), the Century Dictionary criticizes. The fourth definition, that of the English Society for the Improvement of Geometrical Teaching, viz., A straight line is such a line that any part of it, however turned, will coincide with any other part, if its extremities lie in that other part, has merit as a practical description, but pedagogically it is not very satisfactory on account of the difficulty students have in understanding it, and because the definition is not *used* after it is made. Theoretically the Hilbert plan of going at the matter is far superior to the association definition.

Some terms in geometry are used ambiguously, notably circle, straight line and equals. By circle is meant either an area or a circumference, the latter being the usual meaning of circle in higher mathematics. By straight line is meant either an indefinite line, or a line segment or sect. By equal is meant either equal in area or volume, *i. e.,* numerically equal, or equal in all respects, often called congruent. These ambiguities lead to much confusion in the minds of learners. It is probable that the words straight for indefinite straight line, ray for half indefinite straight line, sect for line segment, and congruent for "equal in all respects," will soon be generally adopted.

A difference in the definitions of area and volume as given by various authors and by the dictionaries is of considerable interest. Wells says the area of a surface is its *ratio* to a standard unit of sur-

face. Wentworth, on the other hand, says it is the ratio to a standard unit times the unit of measure. Certainly these definitions do not give the word the same meaning. Which is right? The writer is inclined to think the Wentworth definition correct, for the reason that if one is asked for the area of a field, he does not say, *e. g.*, 11, but 11 *acres*. There is, of course, the same distinction made in the definitions of volume, and the same distinction could be made in defining contents, weight, length, etc.

Perhaps the most striking thing in connection with mathematical definitions is the weakness of their statement in our dictionaries. These definitions are often stated in synonyms when the real thing could just as well be given; they are stated obscurely when it is just as easy to give clear definitions; and they are stated in Latin terms, the language of the schools two or three centuries ago, though the vast body of users of the dictionary do not have the least idea of the meaning of the Latin roots. Thus in defining *number*, Webster's International says "it is a unit or an aggregate of units; a numerable aggregate or collection of individuals; an assemblage made up of distinct things expressible by figures; that which admits of being counted." Compare these definitions with this: a number is one or more units, or ones. They all have this meaning. Only one of the Webster definitions is simple, and it could be simplified still further to advantage by saying that a number is that which can be counted. Notice that number has virtually been defined by a synonym in this definition, since counting would have to be defined. However, counting is a familiar act to every one.

The Old International, latest edition, defines a perpendicular as a line that makes right angles with another, and then a right angle as one that is formed by a perpendicular! The Standard Dictionary does the same thing. If a pupil in a geometry class were to do this, the teacher, metaphorically at least, would box his ears. There is no occasion for defining one of these terms by the other, and then the latter by the former. It would be all right to define either by the other, providing an essential definition were given for the other. In this case the geometries say right angles (or a perpendicular) are formed when one line meets another so as to make the adjacent angles formed equal. The dictionary should do the same. The Century Dictionary says a perpendicular is the shortest distance from a point to a line, and then defines right angle as one formed by a perpendicular. This definition for perpendicular is open to the same objection as the definition of a straight line, which says it is the shortest distance between two points. But the Century evidently does not fall into the silly course of Webster and the Standard. Worcester says a right angle is one of 90°, and defines one degree as one three-hundred-and-sixtieth of a circumference.

We have already seen the pomposity and verbosity displayed by Webster in the definition of number. These characteristics can be duplicated in the definitions of numerous other common words. Thus Webster states that the area of a surface is its "superficial contents": The Century defines area as "the superficies of an enclosed or defined surface space"! Webster defines a proportion as "the relation or adaptation of one portion to another or to the whole, as respects magnitude, quantity or degree." This definition (not intended, of course, as a mathematical one) is hazy enough to suit a mystic. He says *equals* means "exactly agreeing with respect to quantity," which is not bad aside from the fact that every word is Latin with the exception of the two prepositions. Webster defines *ratio* as the relation which one magnitude or quantity has to another of the same kind, and in a note distinguishes two kinds of ratio, arithmetical and geometrical. Now nine thousand nine hundred and ninety-nine times out of ten thousand, by the ratio of two numbers, is meant the geometric ratio or the quotient of one by the other, nearly always the first divided by the second. So far as the dictionary definitions go the reader would be likely to think one definition as important as the other.

The foregoing definitions from Webster are those in the Old International now in wide use over the country. It will be found interesting to compare them with those of the New International recently published which contains numerous new features. Looking for the word *number* we find instead of the four definitions given above, the following two: "The or a total, aggregate, or amount of units (whether of things, persons, or abstract units); arithmetical aggregate; as odd or even number." Now bad as the preceding definitions were, probably every one will say these are inferior in their crude awkwardness. Ideas, for instance, would hardly be included in the parenthesis list, and yet they can be counted when they exist. What the last phrase about odd and even means in its setting does not appear.

Fortunately the bad definition of number just referred to seems to be a very poor example by which to judge the new dictionary. The definitions of ratio and proportions, for instance, unlike in the old dictionary, are above reproach, with a single exception. Under ratio it is said that it is sometimes called the "rule of three." This is evidently a continuation of the old confusion of ratio and proportion. Ratio has only two terms, while rule of three has three, with a fourth implied. Under "proportion" one definition is, the rule of three, which is correct.

Under the word *area* are given the same old definitions which have been handed down from Dr. Johnson's time, hazy in meaning and oozing with Latin roots. Under the word *volume,* on the other hand, strangely enough, is found a simple and correct definition. Thus,

"volume is the space occupied as measured by cubic units, *i. e.*, cu. ft., cu. in., etc.

The new dictionary, unlike the old, defines a right angle, not by a synonym, but by saying it is an angle included between two radii subtended by a quarter circle. Under angle, "right angle" is not given. Thus the new dictionary can not be criticized as the old was. But it does seem a pity that the dictionaries can not give the simple, plain essential definition found in almost all geometry text-books.

Under "straight line," the first definition, a line having an invariable direction, is credited to Newcomb, thus retaining the old weak idea of direction, used in defining parallel lines. Next, Euclid's definition is given, and then the Hilbert axiom as a definition. Legendre's definition is criticized. Of course the reader of the dictionary will not learn the meaning of a straight line from the Newcomb definition, but will learn the meaning "of the same direction" from his knowledge of a straight line. It would have been far wiser to have told what Euclid meant by his definition.

In defining *angle* the same discredited definition of the difference in direction of two lines appears again. Curiously enough the generalized, or trigonometrical, definition of an angle is found not under the word angle at all, but from a cross reference to "Mathematical angle." It is only at this place that the essential quality of an angle as a magnitude is given.

The word *congruent,* for some unknown reason is not given the meaning applied to it in foreign and recent American text-books on elementary geometry.

Under "Parallel lines" we find "lying evenly everywhere in the same direction, however far extended; in all parts equally distant." This is said to be the Euclid idea of the term! Then there is given the modern geometry conception of a point and line at infinity in which parallel lines and parallel planes meet.

Under "Parallel Postulate" is presented a good idea of the difference between Euclidean, Lobachevskian and Elliptical space. Such features as this go far to show that the dictionary is up-to-date in dealing with important ideas of mathematics which the general public has not had a chance to know about heretofore. While this explanation of the parallel postulate deals with one of the most abstruse matters in modern mathematics it is still reasonably intelligible to the ordinary reader. But there have been introduced into the New International the definitions of numerous highly technical mathematical terms whose meaning is quite beyond the ken of all except a very limited number of technical school graduates. Thus, under Dirichlet's theorem is found a triple integral involving perhaps a dozen elements. Similar technical matter will be found under numerous headings.

Evidently in the introduction of such matter the New International has broken away from the long established custom followed by dictionaries and popular cyclopedias, of inserting only what will be fairly intelligible to any well-informed person. This rule probably holds still in other fields of knowledge in the dictionary, but it certainly does not hold in the fields of pure and applied mathematics.

The definitions of ratio and proportion as given by lexicographers in times past strike the present-day reader as curious, and thereby hangs a tale. The old writers following Euclid looked upon ratio and proportion as expressing the relation of quantities, such as lines, and were not ready to admit that ratio could be always a number, since two lines taken at random in general are incommensurable. The old Euclid definition of a proportion (given at the beginning of his Book V.) avoided entirely the question as to whether the ratio of two numbers would always give rise to a number.[1] Whether Euclid's ratios are or are not always numbers, it certainly is true that Euclid cuts irrationals out of his theory of proportion. The modern tendency is to teach that ratios and quantities in algebra are numbers. Certainly in elementary mathematics it is highly desirable for pedagogical reasons that the ratio of two quantities be defined specifically as the quotient of the first divided by the second, a proportion as an equality of ratios, and a quantity in algebra as a number.

Oddly enough, the old writers did not distinguish between ratio and proportion, using the two interchangeably. To understand how this probably came about, it must be observed that proportion is applied to two or more quantities that are thought of as changing, or assuming different values. Thus if two bushels of wheat cost $2.10, five bushels will cost $5.25. Here there are only *two* kinds of quantity, bushels and dollars. With this view of proportion in mind, we can see what Dr. Johnson meant when he said proportion is the comparative relation of one thing to another; ratio; settled relation of comparative quantity. Ash (1775), Fenning (1761) and Webster (1806) give practically the same definition. Bailey (1736), whose work shows he was something of a mathematician, gives the definitions for ratio and proportion now in use, except that he adds at the end that a ratio is a proportion. Webster in his first large dictionary (that of 1828) gives for the definition of proportion "the comparative relation of one thing for another; the identity of two ratios."

It is interesting to find some of the old lexicographers falling into errors of pupils at the present time. Thus Ash defines an angle as the point or corner where two lines meet. Bailey says an angle is the space comprehended between the meeting of two lines, but he hastens to add "which is either greater or less, as those lines incline towards

[1] See Encyclopedia Britannica, article "Geometry."

one another, or stand farther distant asunder." He says perpendicular means level, or a plumb line. Webster, in his 1828 dictionary, says an angle is the space comprised between two straight lines that meet in a point; but he adds also that an angle is the quantity by which two lines diverge from each other.

Though the term *per cent.* in arithmetic is a simple one, the definition of it in practically all the arithmetics and dictionaries wraps it in a fog. A very considerable part of the trouble pupils in school have with percentage is due to obscurity in this definition. This seems like a bold statement to make, but the reader can judge for himself whether it is probably true when he hears the case stated. In simple English the word *per cent.* means *hundredths.* Thus, six per cent. means six hundredths. Instead of giving this definition the books say per cent. means "by the hundred" or "in the hundred." If this is so then 6 per cent. means six by the hundred, which means nothing. Dr. Johnson, who was no mathematician, said that per cent. meant "in the hundred," and all the lexicographers seem to have followed his lead, merely varying his preposition. Webster (1828) says: "In commerce per cent. denotes a certain rate by the hundred. Ten per cent. means ten in the hundred." Webster's International states that per cent. means "by the hundred," "in the hundred." Of course there is a way of giving concrete meaning to these words. Thus 2 per cent. in commission can mean that the agent gets \$2 *in* every \$100 worth of goods he sells, or he gets \$2 *by* every \$100 worth of goods he buys. One sees the obscurity in these phrases, however, very clearly, by using another denominator. Thus, what does 2 by the 8 mean?

In the preface to his dictionary Dr. Johnson says: "Every other author may aspire to praise; the lexicographer can only hope to escape reproach, and even this negative recompense has been yet granted to a very few." Evidently from this lexicographers had a harder time in those early days than they have now; for nowadays it would seem as though the dictionaries were to be regarded as sacred writings, to criticize which would be sacrilege. Had the dictionary makers been criticized more, most likely they would have improved the quality of their work. The writer has heard it said that the dictionaries are as weak in defining other similar technical terms as in their definitions of mathematical terms, and that they are far behind the progressive cyclopedia makers in the quality of the matter they print. It would seem judging from the preceding mathematical definitions as though the greatest opening for a progressive publisher would lie along the way of bringing out a new unabridged dictionary adapted for the use of the mass of twentieth century Americans.

THE MUCH MISUNDERSTOOD FUR SEALS OF BERING SEA

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THE public press has recently engaged in a spirited discussion of the affairs of the fur seals of Bering Sea which is remarkable for the popular misapprehension it discloses of the real facts of this problem, which has been before the public as a national and international issue for a quarter of a century. The recent discussion was precipitated by certain criticisms, by the Camp Fire Club of New York, made against an order of the Secretary of Commerce and Labor for the killing of the annual quota of young male seals during the current season. The order of the secretary was not a new or unusual one. A similar order has been given each season for the forty years in which the herd of the Pribilof Islands has been in the control of the United States, and was in vogue for the half century or more of Russian control.

This order called for the killing of 8,000 of the superfluous young males to secure their skins. It is the way in which the government harvests the product of its fur seal herd. The order is exactly analogous to one which the owner of a herd of 100,000 cattle might give to his agents to drive up and slaughter for market 8,000 young steers. Other analogies might be found in the methods of handling sheep, poultry or any other of our domestic animals from which we derive food or raw material of value and utility.

The fur seal is a polygamous animal, a fact which the Camp Fire Club seems to overlook. Actual enumeration shows that 29 out of every 30 males born are superfluous for breeding purposes. A reasonable proportion of these 29 may be killed for commercial uses without injury to the herd and their withdrawal will have no more effect on the life of the herd than the killing of a like number of steers would have on a herd of cattle.

Moreover, it is not merely feasible and safe to take these animals, but it is beneficial to the herd that they should be removed. To let these young males grow up to adult age would precipitate a condition of fighting and struggle on the rookeries which would be injurious in a high degree to the welfare of the herd. To illustrate by another analogy, the condition which their exemption from killing would produce on the fur-seal rookeries would be exactly like that which would exist on the cattle range if all the young male calves and colts were

A General Rookery View—Breeding Seals.

allowed to grow up as bulls and stallions to contest with one another the supremacy of the herd.

The adult male fur seal is five times the size of the adult female and forty times the size of the young pup of a week old. In the struggles of the bull to defend his harem from other bulls, the young are trampled under foot and the mothers torn to pieces. This condition was very conspicuous on the rookeries in 1896–7, when 5,000 haremless idle bulls fought throughout the season with the 5,000 active bulls in charge of harems. This unfortunate condition in 1896–7 was due to exactly what the Camp Fire Club would have repeated at the present time. In 1891–2–3 there was a *modus vivendi,* pending

A "Pod" of Pup Fur Seals.

the action of the Paris Tribunal of Arbitration, which restricted the killing on land to a few thousand seals for natives' food. The majority of the young males were allowed to escape and grow up as idle bulls, a source of injury and loss to the herd until eliminated by death in contests with one another or by old age. It is in the light of this experience and with a view to obviating its repetition that the order of the secretary for the killing of the superflous young males becomes not merely good business policy, but beneficial to the herd.

The criticism of the Camp Fire Club calls attention to the precarious condition of the herd, which is an admitted fact and one of grave con-

An Adult Male Fur Seal or Harem Master.

Mother Fur Seal and Young.

cern. The mistake is in the implication that the order of the department has anything to do with this condition. As a matter of fact the greatly depleted condition of the herd of fur seals is due to an entirely different cause fully demonstrated and easily understood.

The fur seal gets all its food in the open sea at great distances from land. It resorts to the land only to bring forth and nourish its young to self-dependence. It is resident for this purpose on certain islands in Bering Sea from May to November. The mother seal goes 150 to 200 miles from the rookery to find her food, leaving her young behind, returning to nurse it and again go away to feed. With the storms of winter all classes of animals leave the islands and make a long migra-

A Young Bull Fur Seal.

tion down through the Pacific Ocean to the latitude of Southern California, returning slowly along the coast.

It had been the custom of the Indians of the northwest coast of America from the earliest times to go out in their canoes a day's journey to hunt with the spear stragglers from the migrating herd on its northward journey. It was a precarious business and the number of animals taken was unimportant. In 1879, however, sailing vessels began to be used to take the Indians and their canoes out to the main body of the herd and to enable them to follow its course. This new form of sealing was very successful. The fleet grew in numbers and the catch multiplied until it reached the total of 140,000 skins in a single season. The operations of the fleet gradually extended over the entire migration

YOUNG MALE SEALS—KILLABLE SEALS—HERDED BY THEMSELVES.

route of the seals and included their summer feeding grounds in Bering Sea.

The males being reduced in numbers by land killing, the females predominated in the herd as found at sea. On land the young males are forced to herd by themselves through fear of the adult males. They can be readily distinguished and handled without disturbance to the breeding herd. At sea the sexes can not be distinguished. On the spring migration the mother seal is heavy with young and hence less swift in her movements. On the summer feeding grounds she must feed regularly and heavily through necessity of nourishing her young. As a result the pelagic catch is made up chiefly of the breeding females. Investigations of the pelagic catches of 1895 and 1896 disclosed the fact that 65 to 85 per cent. of its skins were taken from gravid and nursing females. The young of these mother seals died unborn or of starvation on the rookeries. The writer counted 16,000 young fur seal pups which died of starvation on the rookeries of the Pribilof Islands in the fall of 1896 as a result of pelagic sealing for that season. In 1909 he found by actual count that 13.5 per cent. of the birth rate for that season were dead or dying of starvation in August of that year. From 1879 to the present time this hunting of gravid and nursing females has gone steadily on, with the consequence that the herd of fur seals belonging to the United States has been reduced from 2,500,000 animals to less than 150,000 animals.

No other result could be expected from this wasteful and indis-

criminate slaughter. It is not necessary to look for other causes, this cause is more than sufficient. To return to our analogy, suppose the owner of a cattle range should allow the slaughter of 65 to 85 per cent. of his breeding cows with the consequent loss of their offspring. It would simply mean the ruin of the herd of cattle, and pelagic sealing has in like manner brought ruin on the fur-seal herd.

This cause of decline was established for the government in 1898 by a commission of scientific experts. It was pointed out that only by the establishment of an international game law for the high seas which should protect the female fur seal—in other words, the abolition of pelagic sealing—could the herd be preserved and restored. The property involved is a very important one. The fur-seal herd during the first twenty years of its ownership by the United States yielded to the government a revenue of $13,500,000, almost twice the sum paid for the Territory of Alaska. If the conditions of these twenty years held true for to-day—and they would remain true were it not for pelagic sealing—the herd would now be bringing to the government an annual income of $1,000,000.

In the period of fourteen years since the exact relation of pelagic sealing to the reduced condition of the herd was demonstrated to our government, this wasteful and inhuman form of hunting has gone on season by season without interruption. A total of 200,000 gravid

A Fur Seal Harem.

and nursing females have been taken from the breeding stock of the herd. The skins of these animals have been marketed by the pelagic sealers at an average price of $15 per skin, a total loss in cash to the government of $3,000,000, with an actual loss through breeding possibilities of ten times this amount, as the breeding life of the female fur seal is at least ten seasons.

There is abundant ground here for legitimate criticism of our governmental policy in dealing with this valuable industry. There is no occasion to invent grounds of criticism such as those urged against the Secretary of Commerce and Labor for a harmless detail of administration. The responsibility does not, however, rest entirely with the United States. The fur seal question is an international issue. The flags of Japan and Great Britain protect the destructive and suicidal industry of pelagic sealing—an industry which is also on the verge of bankruptcy as a result of the failure of the herd, for it preys on its own capital. Russia also owns an important fur seal herd which has suffered and is suffering in exactly the same way that the herd of the United States has suffered and is suffering. It is the business of these two nations—owners of fur seal herds—to effect an understanding with the two nations which stand sponsor for the pelagic industry to the end that the wasteful slaughter may cease.

Surely the abolition of pelagic sealing, which means the hunting of gravid and nursing female fur seals—exactly analogous to the hunting of the gravid doe or the brooding quail—is a cause which should appeal to and enlist the support of the sportsmen of the Camp Fire Club and all lovers of animals the world over. Every influence of criticism and assistance that can be brought to bear should be directed toward the four great nations—the United States, Great Britain, Russia and Japan—having responsibility for this matter, to the end that this valuable race of animals, the fur seals of Bering Sea, shall be saved to the world.

THE PALEONTOLOGIC RECORD

THE CONTINUITY OF DEVELOPMENT

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CONTINUITY of development in a broad sense hardly calls for discussion here. The paleontologic evidence in its favor is so extensive and so universal that the perfection of the proof is merely a question of the completeness of the evidence. The question for discussion is rather as to the method of race development and specific change—whether continuous, by the slow accumulation of minute individual variations, definite or indefinite, through the influence of natural selection or of other causes—or discontinuous by the sudden appearance of distinct mutations or sports, usually of subspecific or specific value, sometimes of generic value. This question is much debated nowadays, and it would seem that the evidence from paleontology ought to be of the first importance in deciding it.

It is very commonly asserted that this evidence is strongly in favor of discontinuous development. This would mean that new species and even genera appear, as a rule, suddenly at certain levels, and that the record of a phylum is not usually a slow continuous change from one species into another as we pass upward from stratum to stratum; but that one species has a certain vertical range and is then supplanted by another species, this in turn by a third, and so on, each successive stage being an advance over the preceding, but the species overlapping instead of grading.

I think that there is no question but that in vertebrate paleontology the evidence taken at its face value does appear to be very distinctly in favor of discontinuous development. Where we are able to follow a phylum of Tertiary mammalia through a series of strata in one locality, we find that the successive stages appear, as a rule, full formed at certain levels, supplant and replace the more primitive stages, and are in turn supplanted and replaced by more advanced stages. In former years, when the records of locality and level were less exact, it was possible to arrange a series of gradations from one stage to another among the specimens pertaining to a particular phylum, and to assume that this gradation corresponded to the levels in the formation at which the specimens had been collected, and that the specific change was through continuous gradation. The more exact records of locality and level and the more extensive and complete collections in recent years have in general failed to confirm this arrangement. In the great majority of cases, so far as the record shows, new species appear already

distinct, at first sporadically along with the more primitive ones, then more abundantly, finally replacing the older ones altogether. The intermediate gradations occur along with the more typical individuals, but without much definite relationship to intergradation in the succession of strata.[1]

We may illustrate from the evolution of the oreodonts, as these are the most abundant and most completely known of American fossil mammals.

The earliest known representatives of the phylum are *Protoreodon* and *Protagriochœrus* from the Upper Eocene Uinta beds of Utah. Both have very short crowned teeth with five crescents on the upper molar, the fifth crescent quite distinct. The fourth premolar is not molariform. For the next stage we have to shift to another formation, 400 miles away, the White River. In the lowest strata of this formation, the Titanotherium beds, we find *Oreodon, Bathygenys* and *Agriochœrus,* all with decidedly longer crowned teeth, and no trace of the fifth crescent in the molars. In *Oreodon* and *Bathygenys* the fourth premolar is non-molariform, composed of one inner and one outer crescent, as usual among Artiodactyls. In *Agriochœrus* it has become imperfectly molariform with two outer crescents and one inner one. Between the Uinta and White River oreodonts a sharp break intervenes and no intermediates are known. From this point we can trace the subphyla of oreodonts up through a considerable succession in the Big Badlands of South Dakota and the adjoining region. *Oreodon culbertsoni, O. bullatus, Eucrotaphus, Eporeodon, Mesoreodon* and *Merychyus* appear to be approximately successive stages in specialization. The skull is shortened, the teeth become longer crowned, the tympanic bullæ are enlarged, lachrymal vacuities appear, the limbs are lengthened, the feet lengthened and compacted and the thumb is lost. But there is not a continuous intergradation in any of these features as we pass upward in the beds. Oreodons with small bullæ are abundant in the lower and middle White River, the bullæ varying very little in size. A species with medium-sized bullæ occurs occasionally associated with them. In the Upper White River all the oreodons that I have seen have bullæ of large size. The size of the bulla, then, does not increase continuously as we go up through the formation. Another and much more specialized genus of oreodonts, *Leptauchenia,* suddenly appears in abundance in the Upper White River. I have seen a single specimen of this genus from the Middle beds, but it shows no more primitive features than those of the Upper beds. In the Lower Rosebud, immediately overlying the White River, species of *Eporeodon* are common, like

[1] The statements of fact herein contained are based partly upon field experience, chiefly upon the records of some 20,000 specimens of fossil mammals and reptiles in the American Museum collections, most of which the writer has had occasion to examine and identify and to post the field records of level and locality, in the course of cataloguing work.

those of the underlying beds except that some of them have well-developed lachrymal vacuities while others have none. Another new race also makes its appearance suddenly, and in great abundance, in the genus *Promerycochœrus*—structurally derivable perhaps from some of the older oreodons, but not connected with them by intergradations. *Agriochœrus* has disappeared. In the Upper Rosebud the *Oreodon-Merychyus* phylum shows a distinct and marked advance in the length of the crowns of the teeth; lachrymal vacuities are always present, the feet are decidedly more compact and elongate. *Promerycochœrus* disappears entirely and is replaced by a very distinct and more advanced genus *Merycochœrus*. The Leptauchenia series has disappeared temporarily, to re-appear in the Middle Miocene in a more specialized genus, *Cyclopidius,* the last known member of this race.

The Middle Miocene (which should follow the Upper Rosebud) is unrepresented at the locality under consideration (Pine Ridge, South Dakota), but elsewhere overlies beds with an equivalent fauna, and contains *Merycochœrus* in one locality with *Merychyus* (both represented by more specialized species); in another locality it contains instead, *Promerycochœrus* with *Ticholeptus* (allied to *Merychyus*); in a third is found the most highly specialized member of the *Merycochœrus* line, *Pronomotherium.* In the Upper Miocene and Lower Pliocene the oreodonts become much scarcer, and the skulls and skeletons are known only in two or three instances. *Pronomotherium* certainly occurs in Montana; in Nebraska the *Merychyi* are more advanced in dentition, belonging to a distinct subgenus *Metoreodon;* but whether the skulls and skeletons are equally different we do not yet know, nor are we in a position to say whether the change is gradual or saltatory.

But the sum of results in regard to the changes from one stage to another in this best known group of fossil mammals is either that the changes are abrupt, constituting clean-cut faunal divisions marked by the sudden appearance in abundance of a more advanced stage; or else that the new form replaces the older one little by little, but on the whole can not be fairly said to be gradually converted into it by infinitesimal gradations.

This general observation applies, in my opinion, equally well to any abundant group of fossil vertebrates whose phylogeny is sufficiently known to make them worth considering.

If, therefore, we consider that the record is continuous where there is no apparent stratigraphic break, and that the known record really represents what was going on over the entire continent of North America, I do not see that we can fairly escape from the conclusion that new species, new genera and even larger groups have appeared by saltatory evolution, not by continuous development.

But—and here lies the crux of the whole question—we have no

right whatsoever to make either of these assumptions. And without them the argument from paleontology for discontinuous development is almost or quite worthless.

If we consider the general conditions controlling evolution and migration among land mammals, it will be evident, I think, that—

1. The external conditions favoring the evolution and progress of a given phylum will not be uniformly developed all over the world or all over one continent, but will appear first, and be at all times more advanced, in some circumscribed region in one or another continent, or simultaneously in limited areas of two or more continents, similarly situated as to climate, temperature, etc.

2. The animal best able to take advantage of these conditions will be existing at the time (*a*) in one continent or (*b*) in more than one, or (*c*) different animals in different continents may be equally able to adapt themselves to the new conditions.

3. As a result, the new stages of any progressive race will first appear in a limited area and will spread out from that region as the favoring environment spreads, the race at the same time continuing its progress further within that area. This area will be the center of dispersal of the race. Its location will be conditioned by two factors, the early appearance of the new environmental conditions, and the existence of species most able to take advantage of these conditions. Parallelism and convergence in racial evolution will be conditioned by *2b* and *2c*.

4. Progressive change from uniformly warm to zonal climates during the Tertiary must needs have been a great factor in controlling the progress and distribution of Tertiary mammals. As the new conditions appeared first at the poles, the chief centers of dispersal of the animals adapted to them must have been in the northern parts of one or another of the great northern continents.[2] The exact location of the dispersal center for each race would be variously decided by the complex of environmental and faunal relations of each, and might be shifted from time to time by changes in these relations.

5. In the regions distant from the center of dispersal the geological record, if complete, should show the successive appearance of progressively higher types in a phylum, arriving in successive waves of migration, and each new type suddenly or gradually displacing the previous stages. Whether the evolution of a race at its center of diffusion was continuous or discontinuous, the geological record of its progress preserved in any other region would be apparently that of a discontinuous development. It would be not the actual history of its evolution but

[2] To a minor extent in the southern parts of the southern continents, whose restricted area and isolation prevailed in the writer's opinion throughout the Tertiary. There is some evidence, however, along the lines indicated in paragraphs 5 and 6, that Patagonia was the chief center of dispersal of South American Tertiary mammals.

an approximation to it. The closeness of the approximation would be largely measured by the nearness and accessibility of the region in question to the center of dispersal of the race.

6. If the evolution at the center of dispersal was sharply discontinuous this discontinuity would be merely emphasized elsewhere. If on the other hand it was continuous, we should get a near approach to continuity in a complete evolutionary series from a region not remote from the center of diffusion of the race, while the evolutionary series from the same region, of a race whose center of dispersal was remote, would be sharply discontinuous.

7. Applying these principles to some of our American Tertiary phyla, we find that certain phyla which we can be sure were of North American origin, such as the camels, oreodonts and peccaries, do present a much nearer approach to continuity of development than do other phyla which we can be sure were of old world origin, such as the deer, the antelopes or the proboscideans.

I assume that since the oreodonts and peccaries never reached the old world, and the camels did not reach it till the Pliocene, their centers of dispersal were well to the south of the Bering Sea connection with the old world. I assume that since the horses are represented by a double evolutionary series, one in Europe, a closer one in North America, their center of dispersal lay far enough north to spread into Europe on one hand, North America on the other, but that the latter was nearer or more accessible, *i. e.*, their center of dispersal was northeastern Asia or Alaska. On similar grounds the center of dispersal of most of the Tertiary ruminants might be located in northwest Asia, of proboscideans in central Asia, of tapirs in northeastern Asia, of rhinoceroses northeast Asia and Alaska, of dogs in northwest Canada, and so on—a series of indefinite guesses which a careful study of the present geographic distribution, with these principles and the imperfect geologic data in mind, might serve to fix more definitely.

The point at present to be considered is that in such series as the camels, oreodonts and peccaries, we do have a sufficiently close approach to a continuous series to warrant our believing that the true process of their evolution in the center of their dispersal was a gradual one as regards the evolution of genera and higher groups, but for aught that paleontology tells to the contrary, it may have been partly, though not wholly, discontinuous and saltatory so far as the evolution of new species is concerned. But the larger and more complete the series of specimens studied, the more perfect the record in successive strata, and the nearer is the hypothetic center of dispersal of the race, the closer do we come to a phyletic series whose intergrading stages are well within the limits of observed individual variation in the race. The known facts in vertebrate paleontology are, in my opinion, utterly inadequate

to prove whether the development of races was or was not wholly continuous. But I think that the evidence, considered in relation to the imperfection of our knowledge, goes to show that the gaps were not normally wide. In exceptional cases I think we have reason to believe that they were wide (*Otocyon*, for instance), but in these instances the evidence is not that of the paleontological record.

THE CONTINUITY OF DEVELOPMENT

By Dr. T. WAYLAND VAUGHAN

U. S. GEOLOGICAL SURVEY

As nearly every one now admits the validity of the arguments in favor of the derivations of the existing groups of organisms from previous somewhat different organisms through the operation of natural causes, I will not enter upon a discussion of the truth of the theory of organic evolution, nor will I present the results of phylogenetic studies. We will assume evolution to be true, and having made this assumption, the theories of the process and the underlying causes may be discussed.

Only two theories of the process of evolution seem to me possible: (1) Darwin's theory of gradual transformation, or the origin of new species by the gradual augmentation through successive generations of the difference between progeny and ancestors; (2) that brought particularly into prominence by de Vries, the theory of saltation, called by him mutation, according to which the progeny differs definitely, without intergradation, from the parents, and the difference is perpetuated by heredity. There are two theories of the cause of evolution. According to the first, that of Weismann, the cause is within the organisms themselves, new kinds being produced by an inherent tendency to vary, this tendency being due to differences in the germ cells of the two parents; the second theory attributes the cause to the action of the environment on the organisms inhabiting it.

The fundamental problems of evolution can then be resolved into two questions. Is evolution through gradual divergence from the parental type, or by saltation; and is it caused merely by the differences in the parental germ-plasms or is heritable variation produced by the environment acting on the organisms?

As we are all paleontologists, the question may appropriately be put, what light can paleontology throw on these problems? It may perhaps render some assistance in deciding between gradual transformation and saltation, when superimposed conformable beds contain sufficiently abundant faunas, and perhaps the Tertiary marine formations of our southern states will yield important results when studied in

proper detail. Dr. Dall has already traced more or less completely the genealogy of some of the species, and I have noticed certain series of species—the group of *Corbula fossata, C. oniscus, C. wailesiana*, etc., being one of them—deserving thorough study, but the paleontologic work known to me has not as yet been done with the requisite detail to form the basis of an opinion. The principal contribution to our general knowledge of the evolution of organisms that paleontology can make, however, is, I believe, in tracing out phylogenetic lines, and I believe the discovery of the processes and causes of evolution must rest with the experimental biologist. During the past few years very important experimental investigations have been made by several men, and I venture to refer to their results, as I regard paleontology as only an aspect of biology, and think the students in that field should utilize the information gleaned in others.

In the study of variation it has been shown that the selection of fluctuating variations does not carry the species beyond a certain limit, or the extent of the variation is limited, leading to the conclusion that new species can not be produced by this method. I may here refer to ecological surveys and the unreliability of conclusions reached by such researches. Dr. Merriam several years ago presented a paper "Is Mutation a Factor in the Evolution of the Higher Vertebrates?" in which he announced the conclusion that it was not. A critical examination of Dr. Merriam's data showed he had not sufficient information on which to base such a conclusion. His data possess value for the study of evolution in that they indicate material that may be profitably investigated by the experimental method. Attention should also be called to the probable insufficiency of conclusions reached by studying material from successive geologic horizons. For instance, suppose that two usually distinct forms are connected by intermediates. There are no means of ascertaining whether the intermediates represent transition stages between the two forms or are examples of blended hybridism.

That new species may originate through saltation is rather definitely proved; but that it is the only process is not established.

To consider the causes of the origin of new forms: That new forms should originate from the old without the action of some new influence seems to me impossible. The circle of possible combinations of already existent characters could not be transcended, and there would result by crossing only all the combinations possible within definite limits; this would be especially obvious if the de Vries hypothesis of unit-characters be true. Many experiments to determine the influence of various physical factors on individuals showed only somatic changes not of heritable nature and the data accumulated seem definitely to prove that somatic changes, or acquired characters induced through the soma, are not inherited.

Weismann made a great contribution to the progress of biology by focusing attention on the germ cells, and although many of his speculations may be discarded, he was a great stimulator of thought. The work of MacDougal and Tower seems to show how the environment may act on the individual through the germ-cells and induce permanent changes in the progeny.

MacDougal has experimented with species of evening-primroses, by injecting salt solutions into the seed capsules, and summarizes his conclusions in two paragraphs:[1]

The action of reagents having an osmotic and a chemical effect has resulted in the induction of mutants in the progeny of *Raimannia odorata* and *Œnothera biennis*. The mutants thus induced have been tested to the second and third generation and found to come true to their newly assumed characters.

The induction of mutants by the action of reagents is a conclusive demonstration of the fact that hereditary characters may be altered by external forces acting directly upon the reproductive mechanism. The action of the reagents used experimentally is simulated by many conditions occurring in nature.

Tower has conducted a series of experiments on species of beetles belonging to the genus *Leptinotarsa*. He endeavored to influence development by the conditions of moisture and temperature during the germinal stages, and induced changes that were perpetuated in the offspring, the changed offspring at least in some instances mendelizing with the parent species. He presents his conclusions in the following words:[2]

A careful consideration of the various lines of experimentation recorded and of the pedigree cultures and the data from observations in nature irresistibly forces one to the conclusion that in these beetles the only variations of permanence are germinal, and that evolution is through germinal variations. Those germinal variations which arise in nature are permanent and the same variations, of the same degree of permanence, are produced in experiment. The diverse kinds of evidence produced in this and in preceding chapters all go to show that under varying conditions of their surroundings these beetles vary, and that as they become more and more extreme an increasing percentage of striking, permanent variations is found; and as I have just shown, it is possible in experiment to produce in this same way a variety of permanent modifications. From all this evidence, however, there nowhere appears the least trace of a suggestion of any specific action of the conditions of existence, but everywhere there appears only the action of environment as a stimulus, while the response is entirely determined by the organism. All of these variations of purely temporary and of permanent kinds resolve themselves into responses of the organism to the stimuli of its environment, but the nature of the response is entirely determined within the organisms. It is true that different intensities of the same stimuli call forth different responses, but, as is shown in the chapter on

[1] "Mutations, Variations and Relationships of the Œnotheras," Carnegie Institution of Washington, No. 81, p. 90, 1907.

[2] "An Investigation of Evolution in Chrysomelid Beetles of the Genus *Leptinotarsa*," Carnegie Institution of Washington, Publication No. 48, p. 295, 1906.

coloration, the response is entirely determined within the organism, which is adjusted to different intensities of stimuli and reacts according to its own method and on the basis of its own constitution, there being no specific reaction called forth by a given stimulus.

I conclude in the light of these experiments that the production of heritable variations, slight or extreme, represents in these beetles the response of the germ plasm to stimuli. In my experiments these stimuli were external, but there is no *a priori* reason why they might not also be internal.

I desire also to call your attention to some remarks by Loeb:[3]

It is obvious that no theory of evolution can be true which disagrees with the fundamental facts of heredity. It is the merit of de Vries to have shown that a mutation of species can be directly observed in certain groups of plants, and he has further shown that the changes occur by jumps, not gradually. This fact harmonizes with the consequence to be drawn from Mendel's experiments that each individual characteristic of a species is represented by an individual determinant in the germ. This determinant may be a definite chemical compound. The transition or mutation from one form into another is therefore only possible through the addition or disappearance of one or more of the characteristics of determinants. If this view can be applied generally, it is just as inconceivable that there should be gradual variation of an individual characteristic and intermediary stages between two elementary mutations, as that there should be gradual transitions between one alcohol and its next neighbor in a chemical series.

To summarize my own opinions on this subject:

1. I think it very doubtful if paleontology can make any especially valuable contribution to our knowledge of the process or causes of the evolution of organisms, and that this field must be surrendered to the experimental biologist.

2. The results of experimental work indicate that the process is not by the gradual transformation of species, but by saltation. However, the former method has not been shown impossible.

3. Experimental investigations also indicate that the cause of evolution is by the environment acting on an organism capable of responding to it.

4. The causes of evolution are chemical in their nature, and the aid of the chemist is necessary for their thorough elucidation.

[3] "The Dynamics of Living Matter," p. 3, 1906.

CHILD STUDY

By Professor E. A. KIRKPATRICK
FITCHBURG, MASS.

THE sciences concerned with man and the conditions most favorable to his physical, mental and moral development have received a great deal of attention during the last quarter of a century and the results are most striking. It is found that there is tremendous waste of adult human life in preventable deaths and injuries caused by disease and accidents, and immense waste of human energy in idleness and misapplied effort, while morality is shown to depend to a much greater extent than was formerly supposed upon proper housing, feeding and recreative and social opportunities.

It has been demonstrated that the death rate of infants may easily be decreased one half by supplying pure milk to mothers together with directions and the help of nurses in caring for babies. Children are not only dying in entirely unnecessary numbers, but a great many are born defective or caused to become so by improper treatment, so that they must be cared for by the state, at enormous expense. It has also been clearly demonstrated that there is an immense waste of time and energy in the public schools, and that the output of efficient individuals is far below what it should be; also, that because of early employment, lack of play grounds and other causes great numbers are shut up in reform schools and jails, instead of being prepared for performing the duties of citizens.

Although the government has, as yet, done almost nothing in the way of scientific research along these lines, as compared with that which it has done in agriculture, yet enough has already been established by the investigations of private individuals, societies and universities to prove clearly that a large part of this destructive waste and perversion of human life, energy and effort can be prevented by means now known to be efficient.

There is good ground also for the belief that still further enormous saving may be effected after the complex facts of child and social life have been more thoroughly investigated. While the government appropriates millions for researches in agriculture and the diffusion of the results among the people, it was a hard task to get an appropriation of forty-five thousand dollars for human education. In the last congress a bill appropriating fifteen hundred dollars for child study was defeated, and immediately afterward, fifteen thousand was appropriated for

studying clams! Evidently, the public and congress need to be better informed regarding the science of human welfare, and the possibilities of its extension and of the practical applications that may be made of it.

It is certain that researches concerned with children must be of much greater importance for the future welfare of the nation, than those concerned with adults, because the children have a longer possibility of usefulness before them and they are, also, to be the parents and trainers of the next generation, while saving and lengthening the life of adults and enhancing their industrial, intellectual and moral efficiency, can continue for only a comparatively short time. Besides this the same amount of effort produces much greater results in plastic childhood.

At Clark University in Worcester, which, under the leadership of Dr. Hall has been the great center of child study in America, there was recently held a conference on child welfare, attended by representatives of no less than *twenty-seven societies,* whose work brought them in contact with child life. It was the consensus of opinion that there is great need for scientific knowledge of children and for the diffusion and popularization of what has already been discovered by scientific research and found useful in the practical efforts of child welfare societies of all kinds. A national society for furthering scientific research, dissemination of results and cooperation of all agencies concerned with child welfare was, therefore, formed, with Dr. Hall as president.

The departments of agriculture and the bureau of education are ready to do more than they have done along this line, but attempts are being made to have a special bureau formed. It is also thought that bureaus may be formed in the states and in the larger cities, which shall investigate local conditions affecting the welfare of children and the best means of helping them.

Children have been objects of interest and effort since the beginning of time, but the serious, systematic study of their real characteristics has been of very recent origin. They have been regarded as small, weak adults, who are to be brought to adult size and strength as quickly as possible. It is now proved, however, that a child is no more a small adult, than a stalk of corn just out of the ground is like the mature stalk with tassel and ear. If all parts of an infant should grow in the same proportion he would be a monstrosity, instead of a well-developed adult. As a matter of fact, while the body increases in height three times, the legs increase five times and the head only twice. Every organ and part of the body changes its size in proportion to the other parts in developing from infancy to manhood, while the rate of such physiological processes as respiration and circulation, changes to such an extent that diagnoses of health conditions are made on an entirely different basis for infants from that used with adults.

Probably the failure to realize that the digestive processes of children are different from those of adults, has been the cause of more deaths of infants than any other one form of ignorance. Although long known scientifically, this fact is still unrecognized by a large proportion of mothers.

The differences in instinctive tendencies and in emotional and intellectual activities of children and adults are equally great, though less easily expressed in brief terms. It is with this problem of the difference between children and adults that the science of child study is especially concerned. It is because of the nature of these differences, also, that the impracticability of the application of adult methods to the securing of child welfare is evident.

The problem to be solved is by no means an easy one. The characteristics of human beings are so infinite in number that the noting of differences between children and adults is an endless task. This expresses only a small part of the difficulty, however, for what the individual is, depends largely upon the way in which his many characteristics are combined and developed. Moreover, the changes in characteristics and their combination during development are not uniform as the child grows into the man. Not only are some of the changes greater than others and hence their relative proportions modified, but the changes are much more rapid at one time than at another. Again, one kind of change, as for example, growth in height, is taking place rapidly, while growth in diameter is nearly at a standstill. Later the reverse is true. Mentally, the same relation may be found between imagination and reasoning, or ambition and altruism.

Much has already been done in discovering the prominence of certain characteristics at different stages of development, but the details are yet to be worked out. It will never be possible to say for any individual, however, just *when* certain characteristics will be prominent. Even the most fundamental characteristics of physical development, such as the rapid growth in height that occurs near the beginning of the teens, come several years earlier in some individuals than in others. If the racial and family characteristics are known a closer approximation may be made. A child of the southern race and of a family maturing early, even for that race, will have his period of rapid growth much earlier than a child of the northern race, but in the same family individual differences will be found according to which line of ancestry is most prominent in the physical characteristics of the child.

In addition to these native differences, racial, family and individual, it is a well-established fact that in man, as in all other organisms, rate and amount of development are determined, not only by inner tendencies, but also by outer influences of climate, food and exercise and by special accidents or diseases. A child whose growth or development has

been retarded may entirely make up for such retardation by a later period of rapid growth, but the conditions for the rapid development must not be supplied too late or the power to grow is likely to be lost. A period of rapid growth in height after eighteen and in intellectual ability after thirty is rare. Permanent retardation in physical development at an early age produces the dwarf and in mental development, the feeble-minded individual. Many feeble-minded are such simply because they retain the characteristics of childhood at a certain stage of development, instead of developing those of later stages.

The natural order of development in children is very difficult to determine because of countless individual peculiarities. Children of the same age sometimes differ as much in some particulars as one group of children differs from another several years older or younger. This makes it necessary in order to get reliable truths regarding changes with development, either to compare a large number of children of different ages or else to study the same child for many years. Both of these methods have been used and the results of detailed continuous study of individuals, confirm and supplement the results obtained from the study of a large number of children of different ages. In both forms of study care is needed to determine whether the changes that are found to have taken place are due to inner laws of development or are the result of special conditions affecting the development of the individual or the group. On the scientific side, it is important that the inner laws of development shall be determined, while on the practical side it is necessary, if the wisest course is to be pursued, to know what the natural tendencies of development are at each age and how children are modified by special surroundings and modes of treatment.

According to an old view of human nature all natural tendencies should be opposed, while according to another extreme view they should all be encouraged. The medium and common-sense view is that in this, as in other cases, we should know the nature of that with which we are dealing, in order that we may do what we wish with it, at least expense of time and effort.

It still remains to be determined, however, as to how quickly, and by what means, one should seek to bring about changes that are desired. Some, like the gardener, believe in making the conditions favorable for the development of the plant, while others try to force an early development and even pull open the buds before they are ready to blossom. There is a growing belief that nothing is gained by haste and that undesirable tendencies are usually best treated, not by direct opposition and attempts at uprooting, but by utilizing them in harmless ways for the development of opposing tendencies. One illustration will make clear this principle. Dr. C. F. Hodge, of Worcester, found a lot of toads that had been killed by school boys. Although indignant, he, after a

little thought, instead of attempting to directly cure the boys of cruelty, offered a prize for the best essay on toads, containing an answer to the question, "What do toads eat?" The activity of the boys was turned in the direction of observing the actions of toads and for that reason they became interested in animals. This interest was increased as they discovered the great usefulness of toads in gardens. There was after that no need of police help, or "cruelty-to-animal-lectures," to prevent the stoning of toads in Worcester.

Although the science of child study has been developing so rapidly during the last quarter of a century it has, as yet, only begun to be organized into a definite system of knowledge. The demands for definite information from secular and religious teachers and from all sorts of child welfare societies, can be met only in part. Much that is already available, however, is still unknown to these workers and to the public generally, though all these agencies are now profiting, to some extent, by the results of child study and investigation.

Although as a science, child study seeks to discover the common characteristics of all children at each stage of development, yet the first results of child study have been increased emphasis upon the great differences between *individual* children and the need of recognizing those differences.

Psychologists and educators are now seeking to determine just how far individual treatment is necessary or most effective. It has been shown that from fifteen to thirty per cent. of children in any grade can not be seated properly in standard non-adjustable seats and now all progressive schools supply every pupil with adjustable seats, although if the same care were used in seating the children, this is probably not a more effective means of meeting the situation than would be the use of fifteen to thirty per cent. adjustable seats and the rest non-adjustable.

Recent studies of retardation show that from five to thirty per cent. of children in school are "repeaters," or in other words, that the standard courses of study do not fit that proportion of individuals, who are below the average. How many whose needs are not met for the opposite reason, that they could just as well go faster, is not known. It is certain at any rate that whatever general plan of grading is adopted in schools a large number of children *can not* have their needs properly met.

On account of this truth it has been held that all children should be taught individually and this has been attempted in a few places, notably at Pueblo, Colo., by Superintendent Search, who found that the best pupils did four times as much work as the poorest, in the same time. He claimed that, on the whole, better results were also obtained than by class instruction. The first fact has been confirmed by other experiments, the last is still disputed. Another method is to teach only the exceptional children individually, while the others are taught as before

in classes. Still another plan that is now being adopted extensively in large cities, is that of having special schools or classes for each type of exceptional child, the deaf, the blind, the lame, the epileptic, the incorrigible, etc. In Chicago, such children are carefully examined and tested by the experts in the child study department maintained by the board of education, before being assigned to the school that it is thought will best fit the individual. Medical inspection and regular examination of eyes and ears of school children have made very clear the necessity of such special provision for exceptional children.

A large proportion of these will have their needs met in classes and ungraded rooms in the public schools, and others in special institutions, especially those for the feeble minded, but a few need still more individual and expert treatment, such as is given in the school for atypical children at Plainfield, N. J., presided over by Dr. Groszmann and in the psychological clinic and school established by Dr. Witmer, of the University of Pennsylvania. In such schools children who are so different from normal children that under ordinary circumstances or even in special classes they would become a burden upon society may, by individual treatment directed by experts, be developed into happy, intelligent human beings and useful citizens.

Although the first and most evident value of child study has been in the treatment of exceptional children, a marked change has also been brought about both in the popular mind and in the minds of educators, whereby children as children, and not merely as the material out of which men and women are to be made, have now become objects of popular and literary interest. This is strikingly illustrated by current literature. A quarter of a century ago if children appeared in literature it was only incidentally or as foils to adults, but now nearly every issue of popular magazines contains stories or sketches, in which portrayal of child character is the prominent feature. The literary needs of children are also being administered to as never before by writers, librarians and teachers, all of whom are giving careful study to the questions of child nature and the literature that best appeals to it. Children are now recognized as a part of the public with distinct needs that must be cared for.

In our schools, although courses of study are not completely made over so as to fit the needs of children in each stage of development, as they may be some day, yet they have been greatly modified by this new interest in children and by the more complete knowledge of their characteristics at each stage of development. The aim is now, not merely to directly prepare for adult life, but to have them live completely, each stage of development, while making some preparation for the future.

There have been still greater changes in the *methods* of accomplishing results than in the *ideal* of what education should do. Even when

children are being educated chiefly with reference to adult life, it is known that methods suited to adults are far less efficient than those that recognize the peculiarities of child nature. It is now found that the old, analytic, logical methods are not only not the best for young children, but they are the worst possible—far worse than haphazard teaching which leaves the child's mind free to work and develop in its own natural way.

Details can not be given in the space of a single article, but those who know best, believe that if a Utopian social life is ever to be developed it will be largely through the application of the results of researches into child nature. Such research is being carried on in many places and all up-to-date departments of education, normal schools and universities include some instruction regarding the results of such research, while enlightened parents and all progressive workers in institutions dealing with the welfare of children are seeking and using such information. It is to be hoped that the time is not far distant when public appropriations and private benevolence will provide for more extensive prosecutions of research in this difficult, complex and most important of all sciences.

THE RELATIONS BETWEEN TEACHERS AND THEIR PUPILS[1]

BY PRINCIPAL H. A. MIERS, M.A., D.Sc., F.R.S.

PRESIDENT OF THE SECTION

TO preside over this section is to incur a responsibility which I confess somewhat alarms me; for the president may, by virtue of his temporary office, be regarded as speaking with authority on the subjects with which he deals. Now, it is my desire to speak about university education, and for this purpose I must say something of school education; but I would have it understood that I really know little about the actual conduct of modern school teaching. One may read books which describe how it should be conducted, but this is a very different thing from seeing and hearing the teacher in his class; and I fear that personal recollections of what teaching in preparatory and public schools was like from thirty to forty years ago do not qualify one to pose as an intelligent critic of the methods which now prevail.

Human nature, however, has not changed much in the last forty years, and if, in considering the relations between university and school education, I can confine myself to general principles, based upon the difference between boys and men, I trust that I may not go far wrong.

I propose first to consider some general relations between teachers and their pupils, and then explain what, in my opinion, should be the change in the method of teaching, or at any rate in the attitude of teacher to pupil, which should take place when the scene changes from school to university.

First as to general relations between teachers and their pupils.

Educational systems necessarily prescribe the same methods for different teachers, and, being made for the mass, ignore the individual. But happily, in spite of the attempts to formulate methods of instruction and to make precise systems, there are many, and those perhaps some of the most successful, in the army of earnest school teachers who are elaborating their own methods.

Now among all the changes and varieties of system and curriculum there is one factor which remains permanent and which is universally confessed to be of paramount importance—the individuality of the teacher and his personal influence upon the pupil. It is therefore a healthy sign when school teachers who have been trained on one system begin to develop their own methods, for in this they are asserting their

[1] Address to the Educational Science Section of the British Association for the Advancement of Science, Sheffield, 1910.

individuality and strengthening that personal influence, which is the real mainspring of all successful education.

Personal influence is, of course, not only a matter of intellectual attainments; it appears to me, however, that at the present time so much is made of the duty of schools to aim at the formation of character that there is an unfortunate tendency to regard this duty as something distinct from the other functions of a master, and as independent of intellectual qualifications. Among the first qualities now demanded of a master in a public school for boys are manliness, athletic skill and a hearty and healthy personality, and these are often regarded as compensating for some lack of intellectual equipment. I suspect that there is a similar tendency in schools for girls. And yet I think it will be found that the only permanent personal influence is really wielded by teachers who exercise it through intellectual channels, and that those who acquire intellectual authority will generally succeed in training the characters as well as the minds of their pupils.

On the other hand, the master who is not up to the proper intellectual standard will soon be found out by his cleverer pupils, and will lose influence, whatever may be the charm of his character.

The formation of character, so far as it can be distinguished from intellectual training, is largely worked out by the boys themselves in any public school in which healthy tradition and a sound moral atmosphere are maintained, although it is true that these traditions depend upon the character and personality of the teachers.

The educational value of the personal and intimate association with one and the same teacher throughout the school or university career is officially recognized in the tutorial system at Eton, Oxford and Cambridge. It has generally led to excellent results, provided that the tutor possesses the right qualities and that pupil and tutor do not happen to be two incompatible personalities; but the results may be well-nigh disastrous where there happens to be antagonism between the two, or where the tutor does not realize his opportunities and responsibilities. I have known some tutors who only excited a distaste for learning in their pupils, and others who entirely neglected or abused the high trust which had been committed to them; but far more, I am glad to say, who have not only exercised the most profound influence for good on their better and cleverer pupils, but also inspired intellectual interest in the most unpromising of them. Although such a tutorial system does not enter fully into the scheme of other schools and universities, and therefore a student does not usually remain long under any one teacher, it must be within the experience of most persons to have come for a time at least under the influence of a teacher who has inspired real enthusiasm for learning and from whose lips the instruction, that might from others have been a trial, has become an intellectual treat.

It is given to comparatively few to exert this powerful and subtle influence in a high degree, for it is a gift confined to a few rare natures. All the more important is it, therefore, to ensure that an effective personal influence may play its part in the intercourse between ordinary teachers and ordinary pupils in the customary routine of school and university life.

How, then, is the proper personal and sympathetic relation to be established between teacher and pupil, so that the individuality of the one may call out the character and the effort of the other? Those who enquire of their earliest school reminiscences will probably recollect that the teachers who obtained a real hold upon them did so by virtue of the power which they possessed of arousing their intellectual interest. I would ask you for a moment to analyze the character of this interest.

In the young child I believe that it will be found to be mainly that of novelty: with him "this way and that dividing the swift mind," sustained thought, or even sustained attention, has not yet become possible; the inquisitive and acquisitive faculties are strong; and every new impression awakens the interest by its novelty quite apart from its purpose. You have only to watch and see how impossible it is for a young child to keep its attention fixed upon a game such as cricket or football to realize how still more difficult it is to keep his attention fixed upon an intellectual purpose.

To quite young children, except to those who are unfortunately precocious, even an impending examination is not a permanent object of anxiety.

Now contrast the aimless interest which can be aroused in any young child's mind by the pleasure of a new impression, a new activity, or a new idea, with that which appeals, or should appeal, to the more mature intellect of an older student. With him it is not enough that the impression or the idea should be new; if it is to arouse interest it must also direct his mind to a purpose. This is to him the effective interest of his games or sport; in the game the desire to succeed or to win is the animating purpose, just as the expectation of catching a fish is the interest which keeps the angler's attention fixed for hours upon his line. In both the desire is fostered by the imagination, which maintains a definite purpose before the mind.

It is sometimes forgotten that as he grows the pupil is no longer "an infant crying for the light," but has become a man with "splendid purpose in his eyes."

While, therefore, it should be the aim of a teacher of young children to set before them the subjects of their lessons in an attractive manner, so that the novelty is never lost, and not to weary their active and restless minds with too sustained an effort, it should at a later stage be the teacher's aim to keep the object and purpose of the new fact or

idea as constantly as possible in view, and not to distract the ardent mind with purposeless and disconnected scraps of learning.

I ask you to bear this distinction in mind, for it is a principle which may guide us in differentiating university methods from school methods of education.

The distinction need not involve us in a discussion of the "Ziel-Angabe" in elementary education, for that is rather a question of keeping the interest alive during each lesson than of maintaining a permanent purpose in view throughout a course.

The much-discussed heuristic method as applied to very young children does, no doubt, fulfil this object so far as it provides the inquisitive mind with novelty instead of a set task, but so far as it makes the purpose more prominent than the process it may become a method more suited to the adolescent or the adult mind than to that of the young child.

I can fully realize that a most difficult and anxious time for the teacher must be that of the maturing intellect, in the interval between childhood and the close of the school career, when the method and spirit of the teaching must to some extent gradually change with the changing mental characteristics of the pupil. But, whatever may be the right methods of teaching children of ten and young men and women of twenty, many of our failures are due to one or both of two prevalent mistakes: the first, the mistake of teaching children by methods that are too advanced; the second, that of teaching university students by methods that are better adapted for school children. It is with the latter that I wish to deal in this address; but we may in passing remind ourselves that when young men and young women are sent straight from the university to teach children with nothing but their university experience to guide them, it is not surprising that they often proceed at first on wrong lines and as though they were dealing with university students.

The difficulty of divesting oneself of the mental attitude and the form of expression familiar in university circles, if one is to become intelligible even to the higher classes in a school, is betrayed by the unsatisfactory nature of many of the papers set by university examiners to school children. The teachers complain, and rightly complain, that there is often an academic style and form about them which just make them entirely unsuitable for the child.

It is, of course, hopeful that a diploma in pedagogy or some evidence that they have received instruction in method is now generally required of those who are to become teachers in schools. It seems to me, however, somewhat curious that, while efforts are now being made to give instruction in educational method to such persons, no similar effort is made to give instruction in more advanced methods to those who are called upon at the close of their undergraduate career to become uni-

versity teachers, and that in consequence many of them have no method at all.

This may be a matter of comparatively small importance to those who possess not only the necessary knowledge, but also the natural gift of personal influence and the power of inspiring those whom they teach. But for those who are not blessed with these powers it may be almost as difficult to fall into the ways of successful university instruction after the sudden transformation from student into teacher as it is for those who become teachers in schools.

Granting, then, that there should be a radical difference between the ways of school and university teaching, and that there is at present an unfortunate overlapping between the two, let me next consider how the distinction between the intellectual interest of a child and the intellectual interest of a man may guide us in adjusting our methods of teaching when students pass from school to the university.

A tenable, perhaps even a prevalent, view concerning a liberal school education is that its chief purpose is not so much to impart knowledge as to train the mind; indeed, some teachers, influenced, perhaps, in the first instance by the views of Plato, go so far as to think that no subject which is clearly of direct practical use should be taught as such at school. This view they would carry to the extent of excluding many obviously appropriate subjects from the school curriculum, whereas almost any subject may be made an intellectual training; this being a question not of subject, but of the manner in which it is taught. In any event, if the scheme of intellectual training be adequately fulfilled, the period of mental discipline should come to an end with the close of school life, and the mind should then be able to enter upon new studies and to assimilate fresh knowledge without a prolonged continuation of preparatory courses. Indeed, the professed object of entrance examinations to the university is to exclude those whose minds are not prepared to benefit by a course of university study, and to admit only those who are sufficiently equipped by previous training to do so. An entrance examination then should not be merely a test of whether a boy or girl has learned sufficient of certain subjects to continue those subjects in particular at the university; and yet it has unfortunately come to be regarded more and more as performing this function instead of being regarded as a test whether the student is generally fit to enter upon *any* university course. The result is that an entrance examination tends to become a test of knowledge rather than a test of general intelligence; merely one in an organized series of examinations which endeavor to ascertain the advancing proficiency in a limited number of subjects, and therefore tend really to encourage specialization. Specialization is not to be prevented by insisting on a considerable number of subjects, but rather by teaching even one subject in a wide spirit. Another result is that the entrance examination belongs properly neither to the

school course nor to the university course; if it is taken at the age of sixteen the remainder of the school career tends to be devoted to university work, which should not really be done at school; if it is taken after leaving school this means that work is being done at, or in connection with, the university which ought to be done at school. It is certainly true that for various reasons a vast deal of education is now being carried on at the universities which should belong to school life, and moreover is being carried on by methods which are identical with those pursued at school. It is equally true that, owing to the early age at which matriculation examinations or their equivalents may be taken, many schools are now asking that at the age of eighteen or nineteen a school examination may be held which shall be an equivalent not for matriculation, but for the first-degree examination at the university. This would really imply that schools should be recognized as doing university work for two years of their pupils' careers—surely a most illogical procedure and one which supports my contention that there is now very serious overlapping, for it assumes that the work for the first-degree examination can be carried on either at the school or at the university, and therefore that there is no difference in the methods of the two.

An increasing number of candidates actually present themselves from secondary schools for the external intermediate examination of the University of London; in 1904 there were about 150, in 1909 there were nearly 500, such candidates.

There will always be exceptional boys and girls who reach a university standard, both of attainments and of intelligence, long before they arrive at the ordinary school-leaving age. Let them either leave school and begin their university career early, or let them, if they remain at school, widen their knowledge by including subjects which are not supplied by the more rigid school curriculum designed for the average pupils; but let them not cease to be taught as school pupils. It is equally certain that there will also be boys and girls whose development is so slow that they barely reach the university standard when they leave school; yet some among them are the best possible material and achieve the greatest success in the end. For such persons an entrance examination will be required at the age of eighteen or nineteen; but I think it is unfortunate that this should be the same as that which quicker pupils can pass at the age of sixteen or seventeen, for an examination designed for the one age can scarcely be quite satisfactory for the other.

I confess that the whole matter is inextricably involved with the question of university entrance examinations. But to enter upon this here would carry us beyond the limits that I have laid down for myself, and it will be more profitable to decide what should be done at school and the university, respectively, before discussing how the examinations

are to be adapted to our purpose. It will be sufficient for me to say that I have been led to the conclusion that matriculation examinations should be designed to suit the capacity of average pupils not less than seventeen years of age, if they are to test the intelligence of those who are ready to enter upon a university course.

Starting, then, with the principle that the period of mental discipline is closed at the end of the school career, and that those who pass to the university come with fair mental training and sufficient intelligence, let me inquire what should be the relation of university teaching to that which the student has received at school.

Under present conditions the schools which aim at sending students to the universities endeavor to give a general education which will fit their pupils to enter either upon a university course or upon whatever profession or occupation they may select on leaving school. They do not confine the teaching of any pupil to preparation for a special profession or occupation, and they do not generally encourage special preparation for the university.

Now contrast what happens to the pupils leaving such a school to enter a profession or business with what happens to those who proceed to the university. The former pass into an entirely different atmosphere; they are no longer occupied with exercises and preparatory courses which serve a disciplinary purpose; they are brought face to face with the realities of their business or profession, and, though they have to gain their experience by beginning at the lower or more elementary stages, they do actually and at once take part in it.

The university student, on the other hand, too often continues what he did at school; he may attend lectures instead of the school class, but neither the method nor the material need differ much from what he has already done. Should not the break with school be as complete for him as for his school-fellow who goes into business? Should he not be brought face to face with the actualities of learning? After his years of preparation and mental drill at school should he not, under the direction of his university teachers, appreciate the purpose of his work and share the responsibility of it?

Let me take, as an illustration, the subject of history. A public school boy who comes to the university and takes up the study of history should learn at once how to use the original sources. It will, of course, be easier for him if he has learned the rudiments of history and become interested in the subject at school; but, if he is really keen upon his university work, it should not be absolutely necessary for him to have learned any history whatever. In any case, if he has received a good general education and has reached the standard of intelligence required for university work, he ought to be able to enter at once upon the intelligent study of history at first hand; his teachers will make it their duty to show him how to do this; their lectures and seminars will illus-

trate the methods of independent study, and will make the need of them clear to him. If, as is probable, some acquaintance with one or more foreign languages be necessary, he will take instruction in them as an essential part of his history course, in order that he may acquire the needful working knowledge; and to learn something of them with a definite purpose will be to him far more interesting and profitable than to study them only for linguistic training, as he would have been compelled to do at school. After all this is what would be done by his school-fellow who goes into business and finds it necessary, and probably also interesting, to acquire some knowledge of the particular foreign language required in the correspondence of his firm. It will, of course, be all the better for a university student of history to have acquired some training at school in the rudiments of history both ancient and modern, together with the knowledge of classics which is necessary for the former, and of modern languages which is necessary for the latter. But there is not space in the school curriculum for all the subjects that may be required either for the university or for the business of life; the best that can be done is to give a good all-round training and to foster a marked taste or ability where it exists by allowing the boy or girl to include the subjects which are most congenial to them in the studies of their last two years of school life, as I have already suggested, provided that mere specialization is not encouraged at school even towards the end of the school career.

The university course might then become a more complete specialization, but of a broad character—the study of a special subject in its wider aspects, and with the help of all the other knowledge which may be necessary to that purpose.

The university teacher will also differ from the school teacher in his methods, for it will be his business not so much to teach history as to teach his pupil so to learn and study history as though it were his purpose to become an historian; in so doing he will have opportunities to explain his own views and to contrast them with those of other authorities, and so to express his individuality as a university teacher should.

One might choose any other subject as an illustration. In science there should be all the difference between the school exercises, on the one hand, which teach the pupil the methods of experiment, illustrate the principles laid down in his text-books and exercise his mind in scientific reasoning, and, on the other hand, the university training, which sets him on a course involving the methods of the classical researches of great investigators and a study of the original papers in which they are contained, illuminated by the views of his own teacher. He also should awaken to the necessity of modern languages. A boy who, on leaving school, passes not to the scientific laboratories of a university, but to a scientific assistantship in a business or government

department, will very soon find it necessary to go to the original sources and acquire a working knowledge of foreign languages. It is regrettable that under existing conditions a scientific student sometimes passes through his university without acquiring even this necessary equipment. I believe this to be largely due to the fact that he is compelled to spend so much of his time in preparatory work of a school character during the early stages of his university career.

In the literary subjects, and especially in classics, there is, of course, not the same scope for the spirit of investigation which it is so easy to encourage in experimental science. Here the only new advances and discoveries which can appeal to the imagination in quite the same way are those which are being made every year in the field of archeology, and it is therefore not surprising that this subject attracts many of the most ardent students: the methods of the archeologist are more akin to those of the scientific investigator, and his work is accompanied by the same enthralling excitement of possible discovery. For the more able pupils and those who had a natural taste for language and literature no subjects have been more thoroughly and systematically taught for very many years at school, as well as at the university, than the classics; but for the less intellectual children or those who had no natural taste for such studies no methods could well be more unsuitable than those which used to prevail at schools. The grammatical rules and exceptions, the unintelligent and uncouth translation, the dry comparison of parallel passages, the mechanical construction of Greek and Latin verse, produced in many minds nothing but distaste for the finest literature that exists.

With the improved methods now in use Greek and Latin may be, and are, presented to the ordinary boy and girl as living literature and history, and school training in them may be made as interesting as anything else in the curriculum. Upon such a foundation the university should surely be able to build a course devoted to literary, philosophical, historical or philological learning even for the average student, provided that the university teacher undertakes the task of helping his pupils to learn for themselves, and to pursue their studies with a purpose, not merely as a preparation.

The spirit of inquiry which drives the literary student to find for himself the meaning of an author by study and by comparison of the views of others is really the same spirit of inquiry which drives the scientific student to interpret an experiment, or the mathematical student to solve a problem. Only by kindling the spirit of inquiry can teaching of a real university character be carried on. Give it what name you will, and exercise it in whatever manner you desire, there is no subject of study to which it can not be applied, and there are no intelligent minds in which it can not be excited.

The first question which a university teacher should ask himself is,

"Am I rousing a spirit of inquiry in my pupils?" And if this can not be answered in the affirmative it is a confession that the university ideal is not being realized.

Some assert that this principle should also guide school education, and that it should be the first aim of the school teacher to stimulate the spirit of inquiry. My own view is that with young children this should be less necessary; they all possess it, and are by nature inquisitive. It should rather be the object of the teacher not to spoil the spirit of inquiry by allowing it to run riot, nor to stifle it by making the work uninteresting; if the lesson interests them, their inquisitive minds will be quick enough to assimilate the teaching. We are, in fact, brought back to what I have already emphasized—that the real difference between the inquisitive mind of the child and the inquiring mind of the adult is that the former is yearning for information quite regardless of what it may lead to, whereas the latter must learn or investigate with an object if the interest is to be excited and maintained.

I have often thought it an interesting parallel that among original investigators and researchers there are two quite distinct types of mind, which have achieved equally valuable results. There is the researcher who pursues an investigation with a constant purpose and to whom the purpose is the inspiration. But there is also the investigator who has preserved his youthful enthusiasm for novelty and has in some respects the mind of a child; passionately inquisitive, he will always seek to do something new, and very often, like a child, he will tire of a line of research in which he has made a discovery, and take up with equal enthusiasm a totally different problem in the hope of achieving new conquests. I think that a man well known in Sheffield, the late Henry Clifton Sorby, must have been a man of this character. The latter is, perhaps, the most fertile type of original investigator, but it is not the type that produces the best teacher, except for very exceptional and original-minded students; and such teachers do not often found a school of learning and research endowed with much stability. For ordinary students the investigator who pursues his researches as far as possible to their conclusion is the safer guide.

It seems to me suggestive that there are to be found, even amongst the famous researchers, these two types of mind, that somewhat correspond to the mental attitude of the school pupil and the university student. It is as though these great men have preserved a juvenile spirit, some from the days of their childhood, others from early manhood.

It will now be clear that the principle which I am advocating is a very simple one, namely, that the business of direct mental training should be finished at school, and that at the university the trained mind should be given material upon which to do responsible work in the spirit

of inquiry. Preparatory exercises belong to school life and should be abandoned at the university.

All this seems so obvious that it might appear to be hardly worth saying were it not that the methods which actually prevail are so far removed from this ideal.

When, for example, a boy who has not learned Greek or chemistry at school comes to the university and proposes to take up one of these subjects he is generally put through a course of exercises which differ in no essential respect from those which are set before a boy of twelve. In other words, our university method for the trained mind does not really differ from our school method, which is supposed to be adapted to the mind in course of training. Again, boys who have been learning certain subjects for years at school, but are weak in them, have their education continued at the university in the same subjects by the same school methods until they can be brought up to the requirements of a first university examination, which in its character does not differ much from the examinations held at school. Where in this process is to be found the introduction of that spirit of inquiry and investigation which ought to characterize the university course?

It may be asked, In what manner is this change to be introduced, and how is it possible under present conditions, where so many students are all pursuing ordinary degree courses and have no time or opportunity for special work, to provide teachers who can educate them in this spirit, if it is also their duty to get pass students through their examinations? The answer, I think, is that in a university the professors and higher teachers should be, without exception, men who, whatever may be their teaching duties, are also actively engaged in investigation. Their assistants should be teachers who, even if the whole or part of their time is occupied in routine teaching, have yet had some experience in, and possess real sympathy with, modern advanced work under such professors. This is only to be secured by insisting that teachers in a university should all have had some experience of original work, and, just as one of the necessary qualifications for an elementary teacher is some education in method, so a necessary qualification for a university teacher should be some education in research. Any one desirous of qualifying for university teaching should be compelled to devote a certain portion of his student career to research, and the funds of a university can not be better applied than to the retention of the better students at the university for the distinct purpose of enabling them to pursue investigation under the professor for a period of one year after they have completed their degree course, if they have not been able to do so during their undergraduate period. It is not, however, too much to hope that the majority of those who are endeavoring to qualify for the higher educational posts will be assisted to obtain this special experience during their degree course. Under the present

system at most universities, unless the student has been fortunate enough to come in contact with a teacher imbued with the spirit of research who is carrying on his own investigations, it rarely happens that he has the time or the means which would enable him to obtain any insight into the meaning of investigation before he leaves to take up teaching work. The need of post-graduate scholarships for this purpose is very widely felt, and is now frequently expressed. To insist upon such qualifications for all university students is, of course, under present conditions, impossible; but there should be no insuperable difficulty in insisting upon them for those who are to be allowed to enter a university as teachers.

Researchers are born, not made, and it is not by any means desirable that all university students should be cast adrift to make new researches and seek discoveries even under the direction of experienced teachers and investigators. This must depend to some extent upon the character of the pupil as well as of the teacher.

The mere publication of papers may mean nothing, and much that is dignified with the name of research is of no account. To turn a lad on to research, unless it be in the right spirit, may be only to set him a new exercise instead of an old one; to leave him to prosecute an investigation for himself may be to condemn him to disappointment and failure. On the other hand, to carry on any piece of work, whether it be new or old, in the zealous spirit of inquiry, with faith in a purpose, is to insure the intellectual interest of the student; and I can not see why this spirit should not animate all university education, whether it be accompanied by original research or not. The essential condition is that the chief university teachers should themselves create an atmosphere of investigation.

So deep-seated is the belief that nothing must be undertaken without a preparatory course of training that even the best and most brilliant students are frequently discouraged from undertaking a new study until they have been subjected to the mental discipline of an elementary course in it.

I can not refrain from quoting an example which came within my own experience, although I have already alluded to it in another address delivered last year.

When I was at Oxford a young Frenchman of exceptional ability, whose training had been almost exclusively literary and philosophical, and who was at the time engaged on a theological inquiry, expressed to me his regret that he had never learned to understand by practical experience the meaning of scientific work. And when I assured him that nothing was easier than to acquire practical experience by taking up a piece of actual investigation under the direction of a scientific worker, he explained to me that when he had applied for admission to scientific laboratories he had been told that it was useless to do so until

by preparatory courses he had acquired an adequate knowledge of mathematics, physics and chemistry. I offered to make the trial with him, and began with a problem that happened to interest me and that required a new method of simple experimental research. I soon found that a well-trained mind, able to grasp the meaning of the problem and eager to investigate it, could begin without delay upon the experiments, and in the desire to interpret them could find a pleasure and a purpose in seeking the necessary chemical and physical knowledge; whereas to have begun by acquiring this in a preparatory course, with no definite object in view, would have been to set back a mature mind to school methods of training and very possibly to have stifled instead of kindling any real scientific interest.

This is, again, an illustration of my contention that the most special study, if carried on in the true university spirit, is very far removed from ordinary specialization, and involves very wide extension of interest and learning; whereas, if carried on in a preparatory spirit, it is necessarily limited.

In a very short time this student had published three original papers which seem to me of considerable importance, though perhaps on a somewhat obscure subject, and I see that they are now quoted as marking a substantial advance in knowledge.

Of course this is the exceptional case of the exceptionally able student; but I think it illustrates two things—first, the prevalence of the conventional attitude that preparation on school lines is necessary even for the post-graduate student; second, the fact that what is really necessary to the university student is the purpose, and that with this before his eyes he may safely be introduced to new fields of work.

One result of the conventional attitude is that those who have distinguished themselves at school in some subject are often assumed to have a special aptitude in it, and to be destined by nature to pursue the same subject at the university, whereas their school success may only prove that they are abler than their fellows, and that this ability will show itself in whatever subject they may take up. Such students would sometimes on coming to the university be all the better for a complete change of subject, without which the continuance of the school studies too often means a perpetuation of the school methods.

Another result is that when teachers are always playing a somewhat mechanical part in a systematized course, receiving duly prepared pupils and preparing them again for the next stage, such an atmosphere of preparation is produced that many persons continue to spend the greater part of their lives in preparation without any reasonable prospect of performance.

I am well aware that, on the other hand, there always have been and are now many earnest and accomplished university teachers who are pursuing the methods that I advocate, whose teaching is always inspired

with a purpose, whose pupils are stimulated to learn in the spirit of inquiry, and who consequently exercise a personal influence that is profound and enduring. I am deeply conscious how much I owe to some such teachers with whom I have studied and to others whom I have known. But still it does remain true that is not yet the atmosphere of ordinary university education, that it does not yet invigorate the ordinary university student, and that to him the passage from school to the university does not necessarily mean a transition from mental discipline and preparation to mental activity and performance.

The distinction that I have in my mind between university and school teaching may be expressed in this way. At school no subject should be taught to a class as though it were intended to be their life work; to take an example, it too often happens at present, owing really to excessive zeal on the part of school teachers, that mathematics is taught as though each member of the class were destined to become a mathematician; consequently only the few scholars with a real aptitude for mathematics become interested, and the remainder are left behind. On the other hand, at the university each subject should be studied as though it really were the life-work both of teacher and of student. Thus, to take the same subject as an illustration, the mathematical student will attend the full courses of his professors and will follow them with the interest of a mathematician; whereas for the scientific student it will only be in those branches of mathematics which concern him that the interest of his special science will put him on terms of equality with the mathematical student. If I may choose an illustration which is familiar to myself, any student of mineralogy can easily be interested in and benefit by a course in spherical trigonometry, because it is one of the tools of his trade, but to send him to lectures on differential equations would be only to discourage him. On the other hand, the student of chemistry would rather be interested in the latter. To each of them certain branches of mathematics as taught by an ardent teacher afford a real intellectual training, but neither would gain much if compelled to follow a general university course of mathematics designed for mathematicians.

It will be observed that I have endeavored to confine myself to the subject of university education and not to say much, except by way of contrast, concerning school teaching.

I must, however, return to it for a moment, if only to emphasize the danger of that specialization which, since it takes place at school and not at the university, is bound to be narrow, and which is often encouraged in pupils of special aptitude preparing for university scholarships.

That a boy or girl should for a year or even two years before leaving school be practically confined to one subject, and should before entering the university be examined in that alone, appears to me to be contrary

to all the best traditions of school teaching, and to the often-expressed desire of the universities to insure a good general education in those whom they admit. There should, I think, be no scholarship examination which does not include several of the subjects of a normal school curriculum, however much additional weight may be given to any of them. Although it may be necessary that university entrance scholarships in one subject should be given either to encourage its study or to discover those who have a special aptitude, yet, so far as scholarships are intended to be rewards for intellectual preeminence, they should, I think, be directed to general capacity, and not be used as an encouragement to limited study. From what I have already said it will be clear that I do not attach much importance to special preparation at school for those who intend to proceed to the university. If a boy has a very special taste or aptitude, it should have abundant opportunity for displaying and exercising itself at the university, provided only that it has not been stifled, but has been given some encouragement in the school curriculum. I understand, for example, that those who teach such a subject as physiology at the university would prefer that their pupils should come to them from school with a general knowledge of chemistry and physics rather than that they should have received training in physiology. With the present modern differentiation into a classical and modern side, or their equivalents, the ordinary school subjects should be sufficient preparation for any university course if they are not mutually strangled in the pressure of an overcrowded curriculum.

To be fair, however, I must state another view. A very experienced college tutor who has had previous valuable experience as a master in a public school tells me that in his opinion the real problem of the public schools is the "arrest of intellectual development that overtakes so many boys at about the age of sixteen." "There are few public schools," he says, "whose fifth forms are not full of boys of seventeen or eighteen, many of them perfectly orderly, well-mannered and reasonable, in some sense the salt of the place, exercising great influence in the school and exercising it well, with a high standard of public spirit, kindly and straight-living, in whom, nevertheless, it is difficult to recognize the bright, intelligent, if not very industrious, child of two or three years before."

He thinks that there is a real danger of degeneration at this age, owing, for one thing, to the manner in which the boys are educated *en bloc;* up to a certain age boys can be herded together and taught on the same lines without great harm being done, but after a certain time differentiation begins to set in. The school curriculum, however, does not admit of being adjusted to suit the dawning interests of a couple of hundred boys; and he sees no cure for this difficulty except a considerable increase in the staff and a corresponding reduction in the size

of the forms. But he thinks that much may be done by an alteration in the system of matriculation examination, which sets the standard at the public schools. He would make this consist of two parts: an examination coming at about the age of sixteen and well within the reach of a boy of ordinary intelligence and industry, and comprising the ordinary subjects of school curriculum at this age; he would then let the boy leave the subjects from which he is not likely to get much further profit and begin to specialize for the remaining two or three years, say, in two subjects, which would then be the material of the second examination. In this way they would make a wholly fresh start at a critical age, and he thinks that the bulk of the boys would probably find this a great advantage.

I quote this opinion because it shows that an experienced schoolmaster regards it as highly desirable that at a certain period in a schoolboy's career a real change should be made in his curriculum, and I have expressly stated that I find it difficult to express an opinion upon this particular educational period.

What should be the exact nature of the teaching before and after the age of sixteen or seventeen for the mass of ordinary boys I would prefer to leave to the decision of those who are best able to judge. I think it highly probable that there should be a considerable alteration of curriculum at the critical age. But, if a break and change of subject are required at this age, I believe that a yet more complete change is required at the later stage when the boy goes to the university, and that school methods should then be entirely replaced by university methods—not because there is then a natural change in the mental powers of the student, but because it is the obvious stage at which to make the change if we are to abandon preparatory training at all. Should it be proposed that the change ought to be made at sixteen, and that after that age something of the nature of university methods should be gradually introduced, my fear is that this would only lead to the perpetuation of school methods at the university.

An interesting question which deserves to be very seriously considered is the question, What sort of school education affords the best preparatory training for the university? I have often heard it asserted that, if a boy is capable of taking up at the university a course which is entirely different from his school course, he will generally be found to have come from the classical side and not from the modern side. An ordinary modern-side boy is rarely able to pursue profitably a literary career at the university, whereas it often happens that ordinary classical-side boys make excellent scientific students after they have left school. I am bound to say that this is, on the whole, my own experience. It suggests that a literary education at school is at present a better intellectual training for general university work than a scientific education. If this be so, what is the reason?

There are no doubt many causes which may contribute. In some schools the brighter boys are still retained on the classical side while those who are more slow are left to find their way to other subjects; and some whose real tastes have been suppressed by the uniformity of the school curriculum turn with relief to new studies at the university and pursue them with zeal. But the facts do also, I think, point to some defect in the present teaching of school science whereby a certain narrowness and rigidity of mind are rendered possible. This may be partly due to the lack of human interest in the teaching of elementary science; the story of discovery has a personal side which is too much neglected, though it is more attractive to the beginner and might with advantage be used to give some insight into the working of the human mind and character. Moreover, it would form an introduction to the philosophy of science which is at present so strangely ignored by most teachers.

But another noteworthy defect is the absence of that mental exercise which is provided by the thoughtful use and analysis of language.

I believe that the practise of expressing thoughts in carefully chosen words, which forms so large a part of a good literary education, constitutes a mental training which can scarcely be surpassed, and it is unfortunately true that in the non-literary subjects too little attention is paid to this practise. In school work and examinations a pupil who appears to understand a problem is often allowed full credit, although his spoken or written answer may be far from clear. This is a great mistake. A statement which is not intelligibly expressed indicates some confusion of thought; and, if scientific teaching is to maintain its proper position as a mental training, far more attention must be paid to the cultivation of a lucid style in writing and speaking.

The various universities seem fairly agreed upon the subjects which they regard as essential to an entrance examination—subjects which may be taken to imply the groundwork of a liberal education. Among these is English: and yet of all the subjects which children are taught at school there is none in which such poor results are achieved. It may be taught by earnest and zealous teachers; the examination papers are searching, and seem to require a considerable knowledge of English literature and considerable skill in the manipulation of the language, and yet the fact remains that the power of simple intelligible expression is not one that is possessed by the average schoolboy and schoolgirl. It is the most necessary part of what should be an adequate equipment for the affairs of life whether the pupil passes to the university or not, and yet it is on the whole that which is least acquired.

Although it is true that the intelligent reading and study of the great masters should assist in the acquisition of a good style, it is equally true that, if they come to be regarded as a school task, they are not viewed with affection, especially in these days of crowded curricula,

when there is little leisure for the enjoyment of a book that requires deliberate reading. If the modern strenuous curriculum of work and games has abolished the loafer it has also abolished leisure, and has therefore removed one of the opportunities that used to exist for the cultivation of literary and artistic tastes and pursuits by those to whom they are congenial. The art of expressing one's ideas in simple, straightforward language is to be acquired not so much by study as by practise. There is no essential reason why children should write worse than they speak; they do so because they have constant practise in the one and little practise in the other. Our grandparents felt less difficulty in expressing themselves clearly than we do ourselves: of this their letters are evidence. It may have been partly due to the fact that they had more time and encouragement for leisurely reading, though they had not so much to read; but I believe that the letters which they wrote as children were their real education in the art of writing English. Much would be gained if boys and girls were constantly required to express their own meaning in writing. The set essay and the *précis* play a useful part, but do not do all that is needed. Translation does not give quite the necessary exercise. What is required is constant, with certain periods of conscious, practise, and that is only to be obtained by making every piece of school work in which the English language is used an exercise in lucid expression. Very few paragraphs in anything written by the ordinary schoolboy— or, for the matter of that, by the ordinary educated Englishman—are wholly intelligible, and teachers can not devote too much pains to criticizing all written work from this point of view. If we first learned by practise to express our meaning clearly we should be more likely to acquire the graces of an elegant style later. I must add that I believe the training in the manipulation of words would be improved if all children were required to practise the writing of English verse—not in efforts to write poetry, but narrative verse used to express simple ideas in plain language—and I believe that this would enable them the better to appreciate poetry, the love of which is possibly now to some extent stifled by the pedantic study of beautiful poems treated as school tasks.

In such a subject as English composition, in which reform is so badly needed, something, perhaps, would be gained by an entire break with existing traditions—a break of the sort which would be required if it became suddenly necessary to provide for an entirely new type of student.

Now, there is one new and interesting development in which, for the first time, an opportunity offers itself of dealing with a body of students who, although possessed of more than average intelligence and enthusiasm, have not received the conventional training which leads to a university course. The tutorial classes for working people which have now been undertaken by several universities, and which already

number about 1,200 students, are attended by persons carefully selected for the purpose and anxious to pursue a continuous course of study of an advanced standard. In these classes the universities will be compelled to begin new subjects for students of matured minds who have not received the usual preparation, and will therefore necessarily deal with them in a new way. Here, if anywhere, the difference between school methods of teaching and university methods ought to be apparent; and I feel sure that, if university teachers attempt conventional methods with these students, they will be condemned to failure. It is certain that these classes will increase enormously and rapidly, and I have great hope that they will for this reason influence the methods of university teaching in a very healthy manner. In the tutorial classes the teachers will be confronted with the entirely new problem of students who have thought much, and of whom many are experienced speakers, well able to express their thoughts by the spoken word, but who, nevertheless, have received little training, and have had still less experience, in expressing their ideas in writing. Many of the students whom I have met have told me that this difficulty of writing is their real obstacle, and the matter in which they feel the want of experience most acutely. It will be a very valuable exercise for those who conduct these classes to instruct their students in the art of writing simple and intelligible English, and I hope that the necessity of giving this instruction will have a good effect upon the conventional methods of teaching English in schools as well as in universities.

I am conscious that this address is lamentably incomplete in that it is concerned only with the manner of university teaching, and scarcely at all with its matter, and that, to carry any conviction, I should address myself to the task of working out in detail the suggestions that I have made. But this would lead me far beyond the limits of an address, and I am content to do little more than touch the fringe of the problem. Reduced to its simplest terms, this, like so many educational problems, involves an attempt to reconcile two more or less incompatible aims.

The acquisition of knowledge and the training of the mind are two inseparable aims of education, and yet it often appears difficult to provide adequately for the one without neglecting the other. If childhood is the time when systematic training is most desirable, it is also the time when knowledge is most easily acquired; if early manhood is the time when special knowledge must be sought, it is also the time when training for the special business of life is necessary. To withdraw from the child the opportunities of absorbing knowledge may be as harmful as it is unnatural; to turn a young man or young woman loose into a profession without proper preparation is cruel, and may be disastrous.

And so we get the battle of syllabus, time-table, scholarships, examinations, professional training, technical instruction, under all of

which lies the disturbing distinction between training and knowledge.

But, if we inquire further into these matters, I think we shall find that the fundamental question is to a large extent one of responsibility. Left to himself, a boy or a man will acquire a knowledge of the things which interest him, even though they be only the arts of a pickpocket, and will obtain a training from experience such as no school or college can give. If education is to achieve the great purpose of interesting and instructing him while young in the right objects, and also of training him for the proper business of his life before it is too late, is it not mainly a question of deciding when and how far to take for him, or to leave to him, the responsibility of what he is to learn and how he is to learn it? If the teacher bears the responsibility during the period of school training, should not the student have a large share of responsibility in the quest of knowledge at the university?

Now, it is of the essence of responsibility that there should be something sudden and unexpected about it. If, before putting a young man into a position of trust, you lead him through a kindergarten preparation for it, in which he plays with the semblance before being admitted to the reality, if you teach him first all the rules and regulations which should prevent him from making a mistake, you will effectually smother his independence and stifle his initiative. But plunge him into a new experience and make him feel the responsibility of his position, and you will give him the impulse to learn his new duties and the opportunity to show his real powers. It is because I feel that this sudden entrance into an environment of new responsibility is so necessary that I would regard with suspicion any attempt to provide a gradual transition between school and university methods.

In matters of discipline and self-control it is possible and advisable to place responsibility upon school children; in intellectual matters it is not advisable, except for the few who are matured beyond their years. It is, therefore, all the more necessary that this should be done at the moment when they enter the university.

This should be the moment of which Emerson says:

> There is a time in every man's education when he arrives at the conviction that he must take himself for better or worse as his portion; that, though the wide universe is full of good, no kernel of nourishing corn can come to him but through his toil bestowed on that plot of ground which is given him to till. The power which resides in him is new in nature, and none but he knows what that is which he can do, nor does he know until he has tried.

The spirit of independent inquiry, which should dominate all university teaching and learning, is not to be measured, as I have already said, by the number of memoirs published, but it is to be tested by the extent to which university students are engaged upon work for which they feel a responsibility. Visit the universities at the present moment, and, in spite of all admirable investigation which is being carried on,

you will find the majority of students engaged in exercises in which they feel no responsibility whatever. In my opinion this indicates that for them the spirit of true university education has never been awakened. It is, after all, very largely a question of attitude of mind. Any subject of study, whether it be a scientific experiment or an historical event, or the significance of a text, is a matter of interpretation, and to approach it in the university spirit is to approach it with the question, "Is this the right interpretation?" Upon that question can be hung a whole philosophy of the subject, and from it can proceed a whole series of investigations: it embodies the true spirit of research and it opens the door to true learning.

In discussing university education I have not, of course, forgotten that many persons have taught themselves up to a university standard entirely without the aid of professors; indeed, the University of London long ago provided an avenue to a university degree which has been successfully followed by many such persons with the best possible results. But I have endeavored to remind you that at the university as at school for most students the personal influence of the teacher is the important thing; that at the university as at school success in teaching depends mainly on the extent to which the interest of the student is aroused; and that at the university this is only to be done by providing him with a purpose and a responsibility in his work in order that he may understand to what conclusions it is leading him. Until this is done we shall still have university students complaining that they do not see the object of what they are learning or understand what it all means. This complaint, which I have often heard from past and present students of different universities, suggested to me that I should on the present occasion deal with this defect in our customary methods.

In the hope that the attention of university teachers may be turned more fully to this aspect of their work I have ventured to make it the subject of my address.

PROFESSOR NORTON'S LAW OF PROGRESS

By Professor T. N. CARVER
HARVARD UNIVERSITY

PROFESSOR J. PEASE NORTON'S recent article on "The Cause of Social Progress and of the Rate of Interest" contains a notable contribution to economic theory. His analysis of the way in which other factors contribute to the value of the work of the genius is most acute, and his reasoning is, up to a certain point, entirely sound. It is, however, merely a part of the universal law of diminishing returns, and not, as he seems to think, a refutation of that law. Of two factors, X and Y, combined for the production of a given result, if one factor, Y, increases, that increase adds, within limits, to the effectiveness of each unit of X, but correspondingly detracts from the effectiveness of each unit of Y. If, for example, one man, cultivating ten acres of corn, can produce a thousand bushels, he can, ordinarily, if given twenty acres of the same kind of land, produce more than a thousand bushels, say sixteen hundred bushels. Give him the use of still another ten acres, making thirty acres in all, and he can produce still more corn, say two thousand bushels, and so on, until a point is reached when additional land would be of no use at all to him. Up to this point, while every added increment of land adds to the crop produced per unit of labor, at the same time it reduces the crop produced per acre of land. This is a case similar in certain respects to that assumed by Professor Norton, of a genius who makes a labor-saving invention by means of which there is an increase of two dollars per capita in the product of the community. The larger the population the greater the product of this man's labor, or, which amounts to the same thing, the greater the value of the invention. Professor Norton assumes, however, that the product, or the value, of this invention would increase in exact ratio with the population. This he has no right to assume. In fact, inasmuch as the universal opinion of economists is to the contrary, he is under some obligation to support his assumption by definite and positive proofs. It is probably true, at least some economists would agree, that if the population *and the land and natural resources,* and the capital as well, all increase with the population, this joint increase of all productive agents would bring an exactly proportionate increase to the value of the invention. But if the population alone increases, while the land and capital remain fixed in quantity, then the value of the invention, while it may increase, will certainly not increase as fast as the population increases. The twine binder, for example, undoubtedly increased the productive power of labor. Let us assume that at the time of its invention it added two dollars to the product of every person in the civilized world. If the population doubled *and the available*

wheat land also doubled, there is no reason to doubt that the value of that invention would also be doubled; that is, it would add two dollars to the productivity of twice as many people as before. Something like this has probably taken place since the twine binder was invented. But suppose the population had doubled without any increase whatever in the available wheat land. Would that invention still have been capable of adding two dollars to the product of every person, or only a dollar and a half? Or, let the population go on doubling and quadrupling generation after generation, without any increase in the available wheat land, would the twine binder continue indefinitely increasing every person's productivity by two dollars, or only one dollar, a half dollar, etc.? If there is no more wheat land to gather harvests from, is it certain that any more twine binders would be needed with a dense than with a sparse population, or that the value of the invention would increase at all? Has not Professor Norton erred in thinking of labor as the sole factor of production, omitting to think of land as even furnishing a necessary condition of efficient production?

Let us present the argument in another form. An agricultural invention might easily reduce by two dollars the cost of cultivating every acre of land under tillage, thus increasing the efficiency of labor. Obviously then, other things being equal, the more acres there are under tillage the greater will be the total saving effected by the invention. If labor and capital increase in proportion as the land under tillage increases, there is no reason to doubt that the total saving would increase in exact proportion as the land under tillage increased; that is, it would continue to amount to two dollars multiplied by the number of acres. But suppose the labor and capital to remain stationary, while more and more land is brought under tillage, would the saving continue to be two dollars per acre, or would it fall lower and lower? This illustration is given merely to show the necessity of considering all the factors in a problem of this kind, and not a part only.

So long as there are new acres of land being made available, and new funds of capital coming into being, all the results which Professor Norton posits may actually happen. But what does it signify to say that a large population using large areas of land may be quite as well off as a small population using small areas of land, and that an increasing population may be better and better off provided it not only has more and more land but is improving the arts of production at the same time? This, it will be observed, is quite different from saying that we "arrive at the conclusion that the comfort and prosperity of a population tends to increase more rapidly than the population upon which it depends." Until it is shown that this is true, *however small the area of land or the supply of capital,* it is no refutation of the Malthusian theory, and it is not even a criticism of the law of diminishing returns.

As an attack on the Malthusian theory, Professor Norton's argument is of the same character as that which reasons that since big fish eat little fish, and since, as a consequence, every time a big fish is caught the lives of a great many little fish are saved and they are thus allowed to grow to maturity, therefore the more people there are catching and eating big fish the more abundant will fish become, because, for every big fish which is caught, a large number of little fish will be enabled to grow to bigness. ∴ If we will all become ichthopophagi, an inconceivable number of people can subsist and we need not concern ourselves with the law of Malthus.—Q. E. D.

The real conclusions to be drawn from Professor Norton's preliminary analysis, which is really a valuable piece of work if he had not spoiled it by trying to base false conclusions upon it, are as follows: (1) An increasing population may, by reason of the enhancing value of every productive improvement, increase in general prosperity and well-being, *provided* it is not hindered by too great a scarcity of the other necessary factors of production, such as land and capital. (2) In spite of such scarcity, and the consequent operation of the law of diminishing returns, the prosperity may increase, *provided* the arts of production improve rapidly enough to more than counterbalance these disadvantages.

The facts cited by Professor Norton regarding interest rates can all be accounted for without calling in this supposititious "law of progress." A time of rapid invention is naturally a time of great demand for new capital, because an invention is a new opportunity for the use of capital. Therefore, men who see these new opportunities bid high to get possession of capital. This, however, is not a complete explanation of interest. Professor Norton would have difficulty in explaining why an appliance embodying one of these new inventions would sell for less than its anticipated product, without recourse to some of the "agio theories" which he dismisses so slightingly. In new countries, where land is abundant and opportunities are many, there is likely to be an increase of population, because men like to go to such countries. Such countries also furnish abundant opportunities for the use of capital, and men bid high for it in order to attract it. In old and worn-out countries, where opportunities for both men and capital are few, there are likely to be a stationary or decreasing population and low rates of interest.

In conclusion let me say that, though these criticisms seem severe, they are aimed at some of the conclusions which Professor Norton bases upon his analysis rather than upon the analysis itself, which is, let me repeat, an admirable piece of work and destined to increase the already high esteem in which Professor Norton is held by students of economics everywhere.

THE PROGRESS OF SCIENCE

THE HARPER MEMORIAL LIBRARY OF THE UNIVERSITY OF CHICAGO

There was laid recently the corner stone of the library building to be erected at the University of Chicago as a memorial to William Rainey Harper, the first president of the university. Dr. Harper died on January 10, 1906, and shortly thereafter it was decided to secure a fund for a library to be named in his honor. Mr. John D. Rockefeller offered to contribute $600,000 on condition that $200,000 should be given by others, and this amount had been made up by more than 2,000 subscribers.

Under existing conditions in the United States the president of a university has great power and great responsibility; and these are multiplied when he presides over a newly established institution. President White at Cornell, President Gilman at the Johns Hopkins, President Jordan at Stanford, President Hall at Clark, have all impressed their personalities on the institutions whose foundations they laid; but no one has done so more completely than President Harper at Chicago. He was a scholar and at the same time an efficient executive officer of the modern type. It is probable that the machinery of university administration has become too complicated and that we should fare better if there were less concentration of power in the hands of one man; but President Harper certainly showed remarkable skill and energy in the establishment and conduct of a university. Apart from the financial side, where the difficulties were great, he introduced certain educational features, such as continuing the sessions throughout the year and concentrating the courses in a single term, and above all deserves honor for having called to the university leaders in scholarship and science, so that Chicago equals Columbia and is surpassed only by Harvard in the number of men of high standing on its faculties.

It has been suggested that a chapel or a hall for ancient languages might with equal propriety have been erected as a memorial to Dr. Harper. Such buildings would perhaps form more suitable memorials than libraries and laboratories, as they can be naturally built in classic or gothic style and are not likely to require alterations or enlargements.

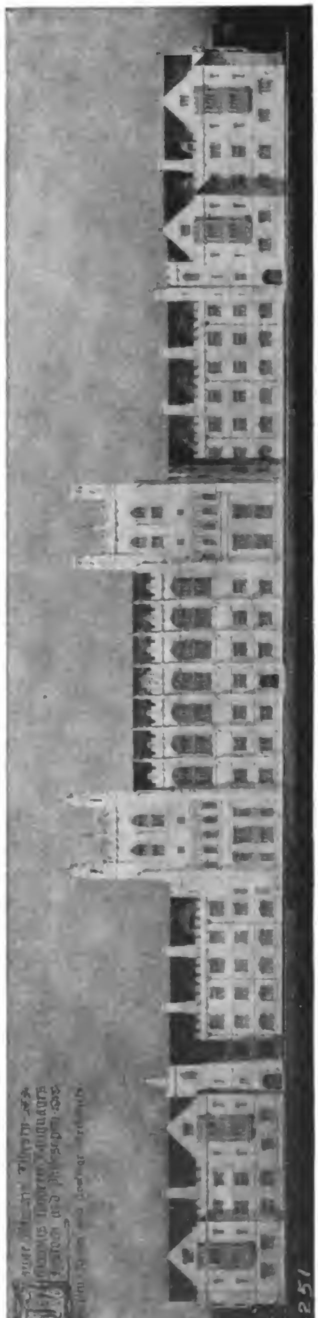
The whole problem of the relation of architectural features to educational uses is complicated. It is desirable for every city to have fine and distinctive buildings, and it is well that libraries and universities should have dignified and worthy settings. The buildings of the Harvard Medical School in Boston and of the Union Theological Seminary in New York City are certainly worth to the community what they cost. It is in a way desirable that the Library of Congress and the Public Library should be the most magnificent buildings in Washington and New York.

But there is another side to this question. It seems unfitting to adapt the needs of a library or laboratory to inherited architectural forms, and to limit their light and growth and usefulness by bricks and stones. The adornments of our college campuses are likely to become monstrosities in the course of a generation. We should plan buildings suited to our needs, and their beauty would then be permanent.

The unity of the university can best be symbolized by a single building and the universal application of the library by a central place in such a building. A modern university might have a

Philosophy History Law Harper Memorial Library Divinity Modern Languages Classics

NORTH FRONT OF LIBRARY BUILDINGS (from model).



Classics Modern Languages Harper Memorial Library History Philosophy

SOUTH FRONT OF LIBRARY BUILDINGS (from sketch).

facade as fine as can be devised, with a great theater and other public halls. Back of this but part of it would be the real university buildings, capable of enlargement to the side and up and down. The lecture rooms and laboratories would be on the unit plan so that partitions could be readily taken out or put in. The library would be in the center, with its seminar rooms extending towards the different departments. Catacombs could be dug as more room was needed for the storage of books, and stories could be added as the library and the university became larger.

The Harper Memorial Library, as shown in the illustrations, does to a certain extent follow this plan. It is to be surrounded by the halls for languages, philosophy and history. But the sciences with their department libraries are separated, and the whole university is scattered over a large area. Students must find hats and coats and travel from building to building to attend a lecture or to consult a book. The historic conditions of universities such as Harvard and Yale may make necessary their extension over an ever-wider territory, but it seems a pity that when universities must erect their buildings from the start, as has been necessary for Columbia and Chicago, there is not at hand sufficient artistic and educational imagination to plan a building that is beautiful because it best serves the purposes of a university.

AGRICULTURAL GRAPHICS

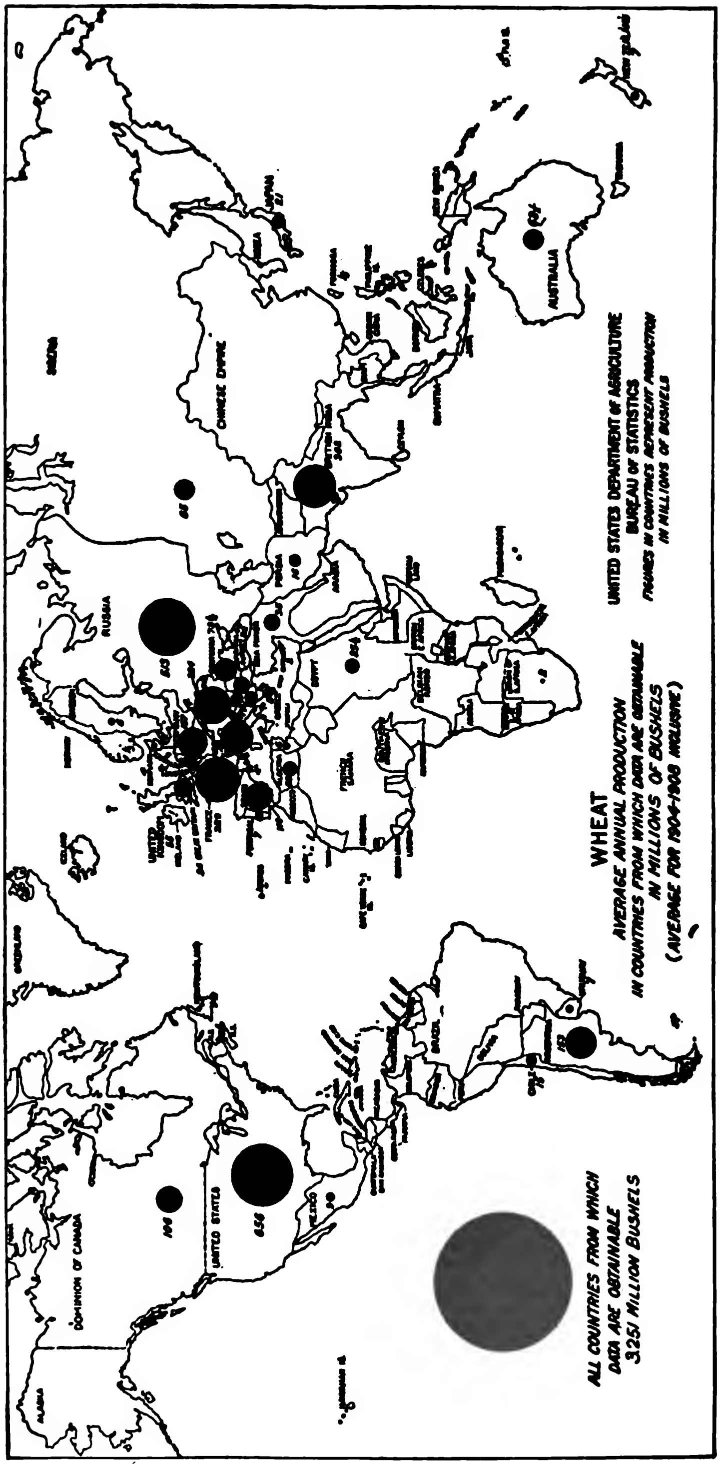
The Department of Agriculture has issued a bulletin, compiled by Mr. Middleton Smith, of the Bureau of Statistics, which contains eighty-eight maps showing graphically the production of crops and farm animals by the states and by the principal countries of the world. Several of these charts are here reproduced. The appeal made to the eye by graphic representation adds to the vividness and permanence of the impression, and such charts have considerable interest and educational value.

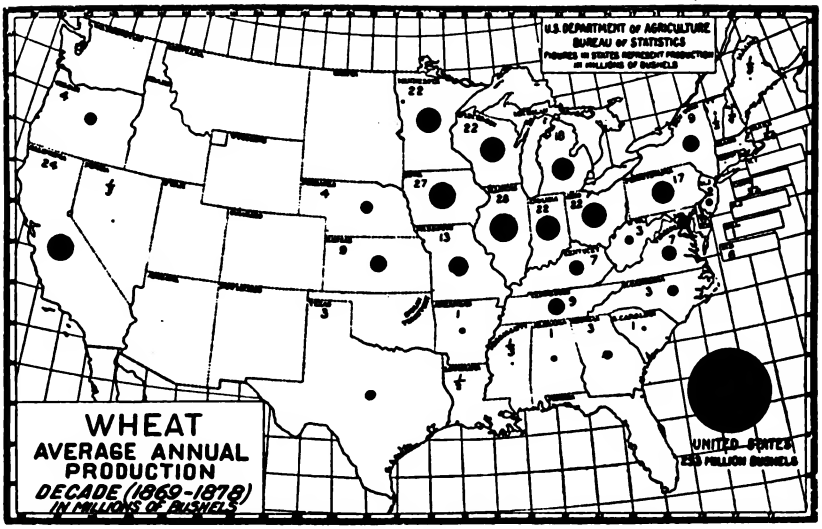
The agricultural products of this country are indeed so large that as-

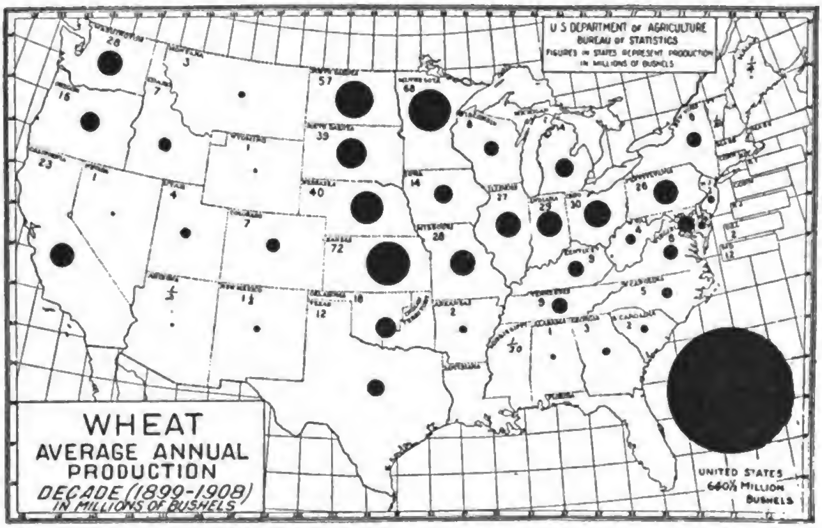
AN AUTOGRAPH LETTER FROM MR. ALEXANDER AGASSIZ.

I wish I could see my way clear to
complying with your request but I am so
overwhelmed with work and with constant
calls on my time that I dare not
add a feather's weight to the load I have to
carry. I can no longer work as I used to and
the claims of outsiders seem to be growing
more and more exacting so that in justice to
what I have to do and to the claims of individuals
and my own interests I am compelled to say
no to any thing new

Yours truly
A Agassiz

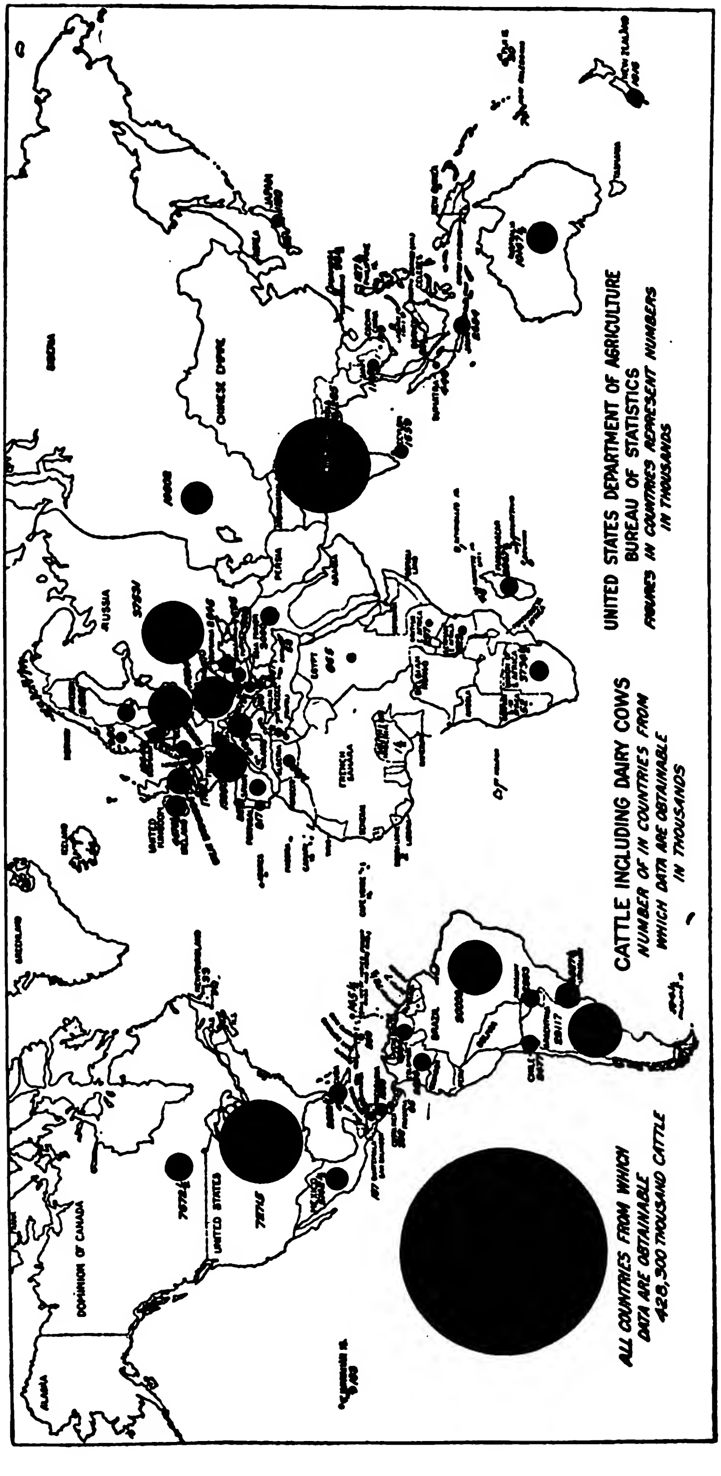

WHEAT
AVERAGE ANNUAL PRODUCTION
IN COUNTRIES FROM WHICH DATA ARE OBTAINABLE
IN MILLIONS OF BUSHELS
(AVERAGE FOR 1904-1908 INCLUSIVE)
UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF STATISTICS
FIGURES IN COUNTRIES REPRESENT PRODUCTION
IN MILLIONS OF BUSHELS
ALL COUNTRIES FROM WHICH
DATA ARE OBTAINABLE
3251 MILLION BUSHELS
DOMINION OF CANADA
UNITED STATES
RUSSIA
CHINESE EMPIRE
AUSTRALIA

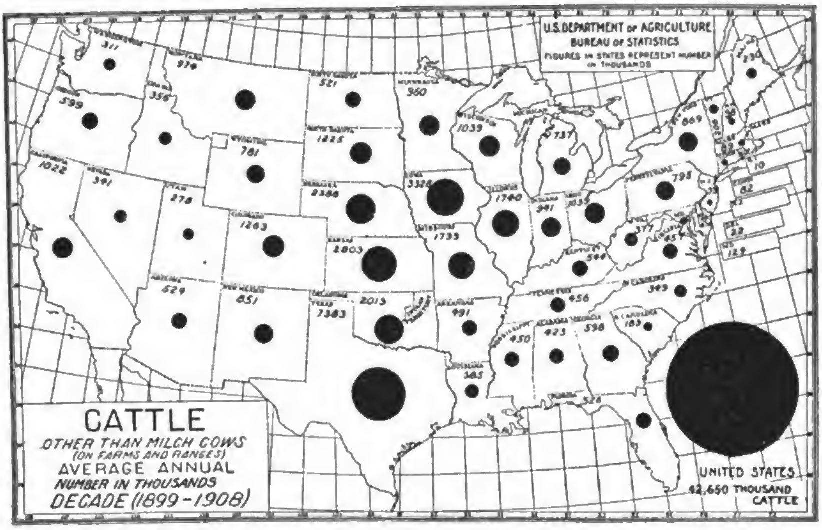


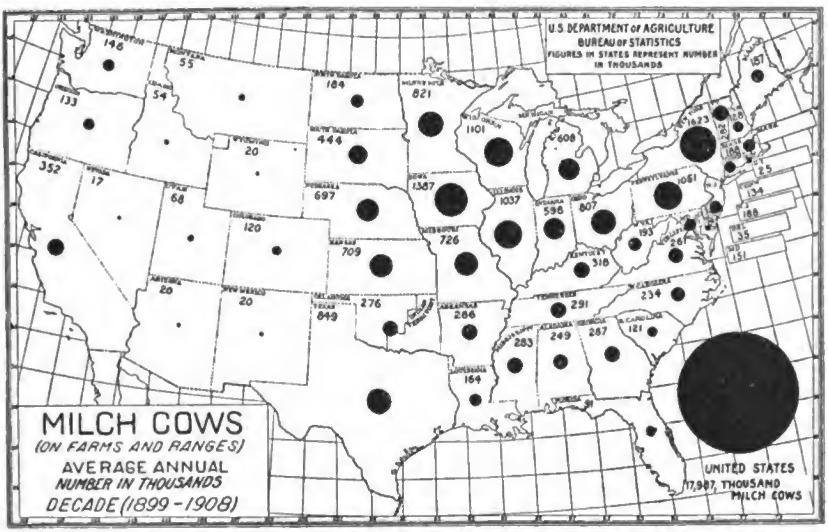


sistance must be given to the imagination if their magnitude is to be appreciated. The corn crop of the United States for 1909 was valued at $1,720,000,000. So much money has not been spent on higher education and scientific research since the first university was established. Each day this crop grows, it increases in value by $14,000,000. An increase of six per cent. in the productivity of the corn crop, such as may result from a moderate amount of research work, perhaps from the efforts of a single man, would be worth a hundred million dollars a year.

Next to corn our most important crop is cotton, the value of which to the farmer last year was about $850,000,000. The United States produces about three fourths of the world's corn and about two thirds of its cotton.


CATTLE INCLUDING DAIRY COWS
NUMBER OF IN COUNTRIES FROM
WHICH DATA ARE OBTAINABLE
IN THOUSANDS
UNITED STATES DEPARTMENT OF AGRICULTURE
BUREAU OF STATISTICS
FIGURES IN COUNTRIES REPRESENT NUMBERS
IN THOUSANDS
ALL COUNTRIES FROM WHICH
DATA ARE OBTAINABLE
428,300 THOUSAND CATTLE
DOMINION OF CANADA
UNITED STATES
RUSSIA
CHINESE EMPIRE
BRAZIL





Wheat, the value of which in 1909 at our farms was $725,000,000, is widely grown over the whole world, and the crop in this country is only one fifth of the total production. Russia, which follows us closely, has about double the crop of France, which is nearly equaled by that of British India. It is a curious fact that we export more wheat than corn, and that the exports of corn have been decreasing. Canada and Argentina are large producers of wheat in proportion to their populations and are consequently large exporters. Only two per cent. of the area of this country is given to wheat, while the percentage is as high as 12 in France and Hungary and still higher in Italy and Roumania. As shown on the charts, the production of wheat in this country has more than doubled in a period of forty years. Kansas and Minnesota are the states now in the lead. New

York and California produce less than formerly.

The crops following wheat in importance are hay, oats and potatoes; there is then a drop to tobacco and sugar, and a further drop to barley, flax, rice, rye and hops. The crops for 1909 were valued at $5,700,000,000, in increase over the preceding year of $869,000,000. These values are, however, in large measure due to increased prices, which have affected agricultural products even more than other commodities. The cost of corn, for example, is more than double what it was ten years ago.

The animal products of the country in 1909 were valued at over $3,000,000,000. The prices here too have increased, but contrary probably to the general belief, much less rapidly than in the case of the cereals. Cattle at the farm have not increased in price in the course of ten years; the wholesale price of beef in New York City has increased 20 per cent. and the retail price 30 per cent.

It will surprise most people to note on the chart that there are in British India more cattle and more dairy cows than in the United States. Texas is followed by Iowa as a cattle-producing state, and New York by the same state in the number of dairy cows. Both the per capita consumption and the exports of meat are decreasing.

SCIENTIFIC ITEMS

We regret to record the deaths of William Harmon Niles, emeritus professor of geology at the Massachusetts Institute of Technology; of Dr. Zdenko Ritter von Skraup, professor of chemistry at Vienna, and of M. Maurice Lévy, professor of mechanics in the Collège de France.

A bronze statue of Lord Kelvin by Mr. Bruce-Joy is to be erected at Belfast.—A monument in memory of Dr. Niels Finsen, to whom we owe the light treatment of lupus and other diseases, was recently unveiled at Copenhagen.—The original laboratory of Liebig in Giessen is to be purchased and preserved as a memorial to the eminent chemist. An anonymous donor has guaranteed 60,000 Marks for this purpose.

Sir William Ramsay has been elected president of the British Association for the meeting to be held next year at Portsmouth. The meeting of 1912 will be at Dundee. The meeting of 1914 will be held in Australia in the cities of Adelaide, Melbourne, Sidney and Brisbane. The commonwealth government has voted £10,000 toward the expenses of the meeting, and the several states will make additional contributions.

At Yale University the salaries of professors and assistant professors have been increased by $49,000 from the alumni fund. The salaries of full professors are to be $4,000 to $4,500 and $5,000, based mainly on length of service, but modified somewhat by university responsibility and personal distinction. In the case of assistant professors the maximum salary is increased to $3,000.